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BRITISH MOSSES.

By the Rt. Hon. LORD JUSTICE FRY, F.R.S., F.S.A., F.L.S.

(*Continued from page* 7.)

WE have seen that Nature has been practising a piece of severe economy in doing without the spore and the whole machinery adapted for the production of spores and substituting a gemma for a spore. We shall now see her going a step further in the same course of economy and doing without the gemma. She will produce protonema from the existing Moss plant without the intervention of spore or gemma.

Fig. 15 represents a leaf of *Orthotrichum Lyelli*, a Moss found both in the old and the new world, on the trunks of trees: from this leaf, and especially from its midrib, are seen growths of protonema, and these gradually change into true roots, and on these roots buds are formed, which buds develop into true Moss plants. At *x* will be seen such a protonema divided into cells by transverse walls—further on the walls are oblique to the line of growth, and the growth then assumes the form of a root—and at *y* is seen a bud destined to produce a Moss plant.

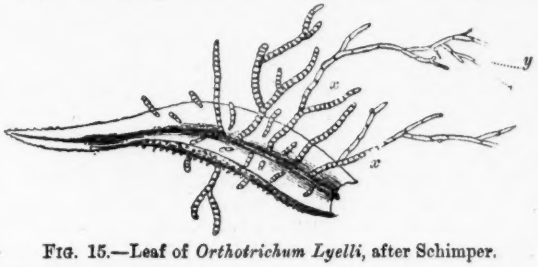

FIG. 15.—Leaf of *Orthotrichum Lyelli*, after Schimper.

Other species of Moss produce protonema from other parts of their structure; sometimes from the small roots or rhizoids, sometimes from the base or the margin of the leaf, sometimes from the stem, and sometimes from the calyptra or veil. These modes of reproduction are referred to in the third column in table B, under the heading "Moss."

But Nature has not yet tired of economy, she will try a yet shorter circuit of life; she will reject the protonema, as well as the spore, and produce on the existing Moss plant itself a bud which shall produce a new Moss plant. Sometimes leafy buds are formed on the true rhizoids, sometimes on the root-like hairs which hang suspended in the air and are known as aerial rhizoids; sometimes bulbs are found on the stem, and from these buds and bulbs fresh Moss plants arise. A yet more direct mode of propagation may exist, viz., the direct production of a Moss plant from a Moss plant; whether, in this case, a bud is first formed or not I do not know, though I should suspect the affirmative. This curious mode of reproduction is shown in Fig. 16, which depicts a plant of *Sphagnum cuspidatum*, on the ends of the loose leaves of which (*a*, *a*) are seen numerous young plants directly arising and alike in all things but size to the parent plant.

FIG. 16.—*Sphagnum cuspidatum*. *a a*, young plants at ends of branches. After Schimper.

The last-mentioned modes of reproduction are epitomized in the last two columns of table B.

There is one reflection which must almost have forced itself on every reader in considering this sketch of the development of Mosses, and of the economies of Nature in the process. The one object of her solicitude is the Moss plant—whatever else be left out, this is always present: Nature may strike off the spore, she may do without the gemma, she may avoid the protonema, but do whatever she may she

always produces the Moss plant, the vegetable growth with its stem and its leaves. To the production of this all else is subordinate; it is the one thing needful.

On the other hand, the one thing which Nature seems desirous to avoid is the sexual reproduction by the concurrence of the two organs of the archegones and the antherids. This is found only in the one mode of growth; every other kind of reproduction by gemma, by protonema, by bud, all of course leave out the whole sporophytic generation.

In the following table, which is far from exhaustive, I have endeavoured to exhibit some of the modes of reproduction, dividing them into those cases in which it takes place with protonema and those cases in which it takes place without.

Table C.—Modes of Reproduction.

		A.—*With Protonema.*	
i.	Spores	in capsule.	
ii.	Gemmæ	on end of leaf	*Leptodontium gemmascens.* *Orthotrichum phyllanthum.* *Grimmia Hartmani.*
		on midrib	*Tortula papillosa.*
		in axils of leaves ...	*Bryum.*
		in balls	*Aulacomnion.*
		in cups	*Tetraphis.*
iii.	Protonema ...	from rhizoids	*Phascum.* *Polytrichum.*
		from aerial rhizoids...	*Dicranum undulatum.*
		from terminal leaves.	*Oncophorus glaucus.*
		from base of leaf ...	*Funaria hygrometrica.*
		from midrib	*Orthotrichum Lyelli.*
		from margin	*Buxbaumia aphylla.*
		from stems	*Dicranum undulatum.*
		from calyptra	*Conomitrium julianum.*
		B.—*Without Protonema.*	
iv.	Leaf-Buds ...	on rhizoids	*Grimmia pulvinata.*
v.	Leaf-Buds ...	on aerial rhizoids ...	*Dicranum undulatum.*
vi.	Bulbs	on stem...	*Bryum annotinum.*
vii.	Young Plants.	at ends of branches ...	*Sphagnum cuspidatum.*
viii.	Leafy Branches.	becoming detached ...	*Conomitrium julianum.* *Cinclidotus aquaticus.*
ix.	Rooting of main axis	... ...	*Mnium undulatum.*

Weismann's Theory.—The consideration of this table, and of the facts which are epitomized in it, is not without its interest in reference to Prof. Weismann's theory of the division of the cells and the plasma of organisms into two kinds: the germ cells and germ plasma endowed with a natural immortality, and the somatic cells and somatic plasma possessing no such endowment. That the Mosses are a difficulty in the acceptance of the theory as a universal truth, the professor himself admits. The evidence of the Mosses seems to amount at least to this: that in this whole group, the highest in this line of development, where the oophytic generation produces the principal plant, and where there are highly specialized organs for the production of spores or germ cells—that in this whole group either there is no effectual separation between the two kinds of plasma, or that the germ plasma is so widely diffused amongst the somatic plasma that every portion of the plant is capable of reproducing the entire organism.

Comparison with Zoological Embryology.—The table will further offer us some points of comparison with animal embryology.

In that branch of physiology, one of the most remarkable facts is what has been called recapitulation, *i.e.*, the summary in the life of the individual of the life of the race, so that the development of the individual tells the development of the race—*e.g.*, the gills of the tadpole tell us of the descent of the Batrachians from gill-breathing animals.

So here we cannot doubt that the protonema of the Moss tells us of the descent of the whole group of Mosses from the Algæ.

Another remarkable fact in animal embryology is the co-existence in exceptional cases of the mature and the immature form: so the axolotl retains both gills and lungs throughout its life. In like manner some Mosses retain their algoid protonema throughout life.

The Phascum or Clay Moss is a conspicuous instance of this curious fact: it is depicted in Fig. 17. It is a Moss of a not very high organization. The leaves grow close to the ground and the stem is very short. In like manner the sporangium (*a*) is almost sessile, and is seen almost enshrouded in the leaves, true rootlets or rhizoids (*r*) attach the plant to the ground, and the protonema (*p*) from which the plant has arisen survives and remains attached to it during the whole life of the plant. This protonema often exists in great quantity in the clay banks or fields where the Phascum dwells, and forms a sort of tangled mat.

Fig. 17.—*Phascum cuspidatum. a*, capsule; *r*, rhizoids; *p*, persistent protonema. After Schimper.

Again, in zoological embryology, an attempt is often found, to use the language of Prof. Milnes Marshall, "to escape from the necessity of recapitulating, and to substitute for the ancestral process a more direct method."

In like manner we have already seen to how great an extent Nature has adopted the system of short-circuiting in the reproduction of the Mosses; for in every mode of reproduction, except that through sporogone and spore, a shorter circuit is travelled. We have seen how in every case Nature seems to leave out the sexual reproduction if she can help it, and directs her whole attention to the production of the vegetative organism—the Moss plant in the popular sense—which she never omits.

Another point of comparison arises, but this time it is one of contrast between the embryology of the two kingdoms.

In animals, to again quote Prof. Milnes Marshall, "recapitulation is not seen in all forms of development, but only in sexual development, or at least only in development from the egg. In the several forms of asexual development of which budding is the most frequent and the most familiar, there is no repetition of ancestral phases, neither is there in cases of regeneration of lost parts."

In Mosses, on the contrary, the table last given shows that in most of the modes of reproduction the ancestral form, the algoid protonema, is retained and reproduced, whereas in the growth from a sexual cell, *i.e.* in the sporogone, the ancestral form entirely disappears.

Organization.—I now propose to describe somewhat more in detail certain parts of the structure of a Moss.

The *stem* of Mosses is, as we have already seen, very variable in size. Sometimes, as in the Phascum (Fig. 17), the whole plant is almost sessile; in other cases, as in the Polytrichum (Fig. 2), it attains to a very considerable length. In some Mosses inhabiting water, the length of the plant reaches to feet. In our flowering plants the stem is supported by the presence of fibro-vascular bundles, *i.e.*, fibres arranged in combination with tubes along which fluids can and do pass. But with the exception of one family, the stem of the Mosses, like all the other parts of the plant, is constituted of cells alone, and consequently the circulation of fluid in them appears to result entirely from the passage of fluid through the walls of the cells. Hence their close dependence on the presence of moisture; hence in dry weather they fade and droop, and with the return of moisture assume their wonted appearance.

The exception to which I have referred exists in the family of the Polytrichaceæ, of which the genus Polytrichum is the foremost (Figs. 2 and 3). In that kind of Moss the stem of the plant and the stalks that support the

capsules are of a firm, almost woody, structure, and give to the whole plant a different character to that of most of the Mosses. This peculiarity of the Polytrichum has, so to speak, enabled it to play a greater part in the world than most Mosses. Gilbert White tells us that the foresters of his neighbourhood made "neat little besoms from the stalks of the Polytrichum, common or great golden maiden hair, which they call silkwood, and find plenty in the bogs. When this Moss is well combed and dressed, and divested of its outer skin, it becomes of a beautiful bright chestnut colour, and being soft and pliant is very proper for the dusting of bed curtains, carpets, hangings, &c." But long before the dwellers in Wolmer Forest discovered this use for this Moss, it had been known to the pre-historic dwellers in our island, and had, it appears, been used by them to adorn themselves or their wives (themselves most likely). Curious fringe-like objects plaited of the stems of this Moss have been discovered in a crannog, or island fort, at Lochlee, in Ayrshire, attributed to that pre-historic period which has been called the late Celtic period. Furthermore, it is perhaps due to this fibrous character of the class that the earliest Moss of which we have any record in the strata of the earth appears to be one of the Polytrichaceæ.

The roots or rhizoids of the Mosses are distinguished by the minuteness of their growing ends, by their pliancy, and by the presence on their exteriors of a balsamic or glutinous deposit. To these points of structure they owe their capacity to insinuate themselves into the minutest crevices of rock, to get, for instance, amongst the particles of the oolites, and also to fix themselves in the shifting sands of the sea-coast, and by so fixing themselves to give fixity in return to the sand, and so tend to produce the sand-dunes in many parts of the coast. At some parts of the Northumbrian coast the *Racomitrium canescens* may be found buried deep in the sand, from which it can scarcely be detached; and in like manner the sand-dunes of Holland and the west of France have in many places been fixed by Mosses. The forests of firs on the North Sea and the Bay of Biscay thus owe their place of abode to humble Mosses.

Leaves.—When we examine the leaves of Mosses and compare them with the more familiar forms presented to us by the phanerogams, we find ourselves in a new world, and the interest with which we view them is increased when we remember that, according to the view usually accepted, they are, so to speak, a unique phenomenon; they are not the descendants of any earlier leaves nor the ancestors of any later ones; they appear thus once, as it were, in the history of the vegetable kingdom, and advance no further. They possess something of the charm which an ἅπαξ λεγόμενον exercises over the mind of a philologist.

We may first note what they are not. They are never opposite, never whorled, never on leaf-stalks, never truly veined, never lobed or compound, never furnished with epidermis or stomata.

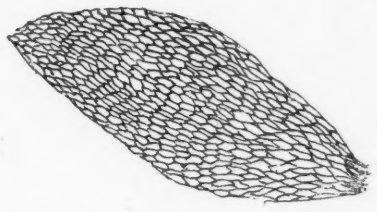

FIG. 18.—Leaf of *Hookeria lucens*, magnified, after nature.—A. F.

When we turn to consider affirmatively what Moss leaves are, we find them in some cases characterized by an extreme simplicity of form. They are single plates of similar cells without midribs, without veins, and without border.

The accompanying Fig. 18, representing a leaf of the beautiful Moss, the Hookeria lucens, is an illustration of this form of leaf, and the Figs. 19 and 20 will show more highly magnified the structure of the component cells in a young and old leaf, and the grains of chlorophyll in the cells. In the old leaf a tendency will be observed in these grains to place themselves along the walls of the cells so as to produce the effect of thickened walls.

FIG. 19.—Cells of young leaf of *Hookeria lucens*, after nature.—A. F.

FIG. 20.—Cells of old leaf of *Hookeria lucens*, after nature.—A. F.

The leaves of Mosses stand in immediate connection with the atmosphere, absorbing moisture from it when moist, and shrinking and shrivelling when the air is dry. In some cases they are characterized by a marked difference in the form of the cells in the different parts of the leaf, and again in other cases by the unequal distribution of chlorophyll; in other cases we come across strange forms, the like of which we hardly know in the phanerogams; such are the thick border and double rows of teeth in some of the genus Mnium, the parallel plates in Polytrichum; and, stranger still, the third flange of the leaf in Fissidens, the true homology of which has proved a *crux* to bryologists.

A drawing of the leaf of *Fissidens adiantioides* is shown in Fig. 21—a thickened line of cells down the middle of the leaf assumes very much the appearance of a midrib, and on the right hand side occupying the lower half of the leaf is seen a third flange to the leaf, attached at its upper part to the leaf in an oblique line and after that to the vein or midrib of the leaf, so that in that part of the leaf there are, as it were, two sheets or plates instead of one. Various theories of the homology of this part of the leaf have been suggested. By some it has been thought to result from a vertical splitting of the leaf; but each of the two plates where they are doubled is of an equal thickness to the rest of the leaf. Some have suggested that the double portion is alone the true leaf and the rest an outgrowth, but this seems a violent assumption. Others, again, have suggested that the additional lobe is a stipule arising on the opposite side of the stem, which has become adnate with the leaf. Some of these suggestions carry conviction with them.

FIG. 21.—Leaf of *Fissidens adiantioides*, showing *a a*, the third flange. After Schimper.

In some cases the leaf is produced into a long thread or beak, devoid of chlorophyll, and often with indented or toothed edges. This structure is found chiefly in Mosses living on stones and rocks, and in dry situations, such as Grimmia and Racomitrium, and the presence of these long white threads or beaks gives a grey tint to the whole Moss, and in places where the Moss is predominant (as, for instance, some parts of Dartmoor and North Wales, where Racomitrium abounds) a grey tint to the whole landscape. These long hairs and prominences, especially when armed with lateral teeth, no doubt retain the moisture which is necessary not only for the vegetative life of the Moss, but also for the process of reproduction by archegones and

antherids; hence it probably is that this form of leaf prevails in Mosses living in dry situations, just as the thick leaves of succulent plants are found in similar situations.

The capsule.—Of all the organs of a Moss plant, the capsule which produces the spores is, perhaps, the most peculiar and characteristic. If the reader will refer back to Figs. 1 and 2, he will see the capsules (*c* in Fig. 1, *a* in Fig. 2) borne on the end of the long stalks (*s*). The capsule, as is shown in Fig. 2, is covered with a delicate veil or calyptra, which is shown as removed at *cal*. This veil is the remains of the archegone, borne up by the stalk or seta in its upward growth. In the case of the Polytrichum, it is covered with a thick coating of depending hairs, a circumstance which gives its name to the genus. When the veil is removed, the capsule itself is disclosed (*c* in Fig. 2), surmounted by an operculum or lid (*o*), which fits on to the top of the capsule like the lid on a box. The capsule with the lid removed is shown at the letter *c'*.

If the reader will refer back to table A, he will find that the Acrocarpous Mosses, *i.e.*, those which produce their capsules at the end of the axis, are divided into Stegocarpæ and Cleistocarpæ. We are now in a position to appreciate this distinction. In the Cleistocarpous Mosses, the capsule is never differentiated into the two parts of the true capsule and lid; it remains always as a closed capsule until the walls decay or break, and so emits the spores which it contains. Of this class, the clay Moss or Phascum (Fig. 17) is a familiar example. As a whole, this class is less highly organized than the Stegocarpous Mosses—such as the Polytrichum—where the capsule, originally a single organ, becomes differentiated into the two parts already described, and the spores are retained in the capsule dry and snug until the ripened lid falls off and allows their escape.

Fig. 22. Peristome of *Tetraphis pellucida*, after Schimper.

In some cases, the orifice of the capsule is formed by a smooth edge or lip; but in other cases this orifice is surrounded by a girdle of teeth of varying number, form, and colour, so that the study of the peristome, as this girdle is called, presents a continued variation of objects of beauty and interest. Fig. 22 exhibits the peristome of the beautiful little Moss the *Tetraphis pellucida*, to which I have already referred. No simpler form of peristome can be found than this, exhibiting four teeth in a single ring. Fig. 23 is part of the peristome of the *Fissidens adiantioides* magnified, and shows two phenomena common in peristomes, (1) the division of the teeth at their free ends, and (2) the presence of transverse markings, generally of a darker colour than the intervening spaces. A tooth thus marked is said to be trabeculated, *i.e.*, marked by trabeculæ, or little beams.

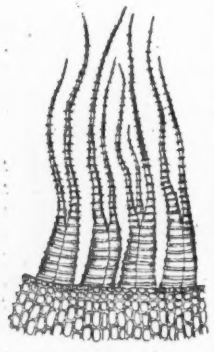

Fig. 23.—Part of peristome of *Fissidens adiantioides*, magnified. After Schimper.

Fig. 24. Peristome of *Tortula ruralis*, magnified. After Schimper.

In one considerable family of Mosses, some of which are very common on the tops of our walls, the teeth are hair-like in length and delicacy, and are twisted into a curious scroll like a lambent flame of fire. Fig. 24 represents one of these twisted peristomes from which the genus especially characterized by it has received from some botanists the name of Tortula.

Again, in another form, which exists in Polytrichum, the teeth assume a very different appearance and connection. To make this intelligible I must refer to a portion of the structure of the capsule to which I have not hitherto referred, the columella, or little column, a central stem which occupies the very axis of the capsule; this, in Polytrichum, emerges from the mouth and expands into a tympanum or drumhead, and the teeth arising from the lip of the mouth join and support this drumhead, leaving interspaces between them something like long narrow windows under the flat roof of a circular tower, through which the spores escape. Fig. 25 is a representation of this singular structure, in which *p*. marks the place of the peristome or girdle of teeth: these are seen to be attached to *t*., the tympanum, into which the column has expanded.

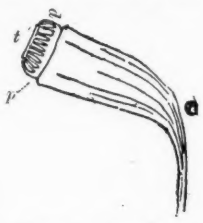

Fig. 25.—Capsule and peristome of *Polytrichum*. *p*. peristome, *t*, tympanum, after nature.—A.F.

I have hitherto spoken of the peristome as consisting of one girdle of teeth; often it is double as in the great genera Hypnum and Bryum, and then the teeth often reach the number of sixty-four. In one foreign genus (Dawsonia) there are as many as four circles of teeth.

The accompanying Fig. 26 is a diagram intended to assist the reader in gaining a general notion of the structure of the several parts of a capsule with a double peristome: it is a diagram only of a section of an ideal capsule, and not a picture or representation of any existing capsule. The reader who will carefully inspect it will learn what to look for when he first holds a capsule in his hands, and may get some assistance as regards the technical language of bryology. He will see the calyptra, or veil (*cal.*), the remains of the original archegone; he will see the operculum, or lid (*oper*),

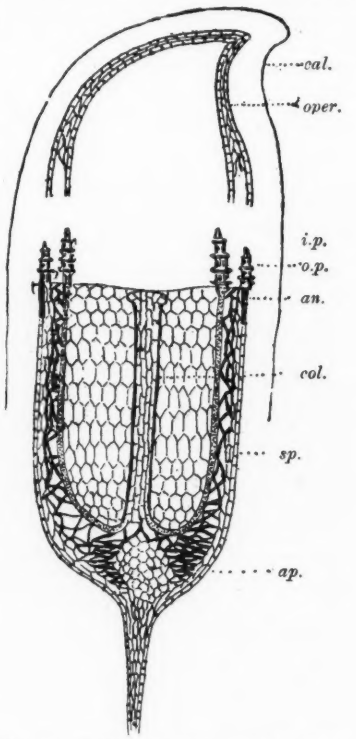

Fig. 26.—Diagrammatic section of a capsule. *cal.*, calyptra; *oper.*, lid or operculum; *ip.*, tooth of inner peristome; *op.*, tooth of outer peristome; *an.*, annulus or ring; *col.*, columella; *sp.*, wall of the actual spore sac; *ap.*, region of the aphyse.—A. F.

severed from the capsule itself; he will observe the double peristome (*i.p.* and *o.p.*), the outer teeth consisting of a prolongation of the outer coat of the capsule, the inner teeth arising in like manner from the wall of the inner sack or spore case, or sporangium (*sp.*); he will observe an interspace between these two sacks filled with cellular tissue; he will observe in the interior of the sporangium the cells which become spores with the maturity of the growth; and in the middle of the diagram he will notice the columella or column. At the base of the capsule he will see the region (*ap.*) which, when swollen or enlarged, gives rise to the apophyse. All these parts are subject to a great range of variation, but this diagram may nevertheless, I hope, prove of some assistance to those beginning the study.

The object served by the complicated structure of the peristome is not, perhaps, very certain, but it seems to be intended to secure the retention or exclusion of the spores from the spore sac in such conditions of the atmosphere as will best conduce to their germination. In the Gymnostomous Mosses (*i.e.*, those without peristome) it is observed that the spores sometimes germinate within the capsule, an event which is probably adverse to the prospects of the race. The following table will illustrate, in a few cases selected as illustrations, the different behaviour of the teeth of the peristome under different hygrometric conditions, and suggests what is the probable advantage in each case :—

TABLE D.

Genus.	Condition of teeth		Reason suggested.
	in dry weather.	in wet weather.	
Bartramia ...	Erect ...	Convergent	That spores require *dry* weather when first emitted.
Orthotrichum	Erect or reflexed ...	Ditto ...	Ditto
Funaria ...	Reflexed ...	Ditto ...	Ditto
Bryum ...	Convergent...	Expanded	That spores require *wet* weather when first emitted.
Fissidens ...	Ditto ...	Ditto ...	Ditto

The motion of the teeth of the peristome appears to be due to the action of the annulus, a ring of specialized cells which surrounds the mouth of the capsule at the base of the teeth: and the opposite ways in which these cells act in the same condition of moisture in different genera is a remarkable circumstance.

To anyone who studies the subject, the immense variety as well as beauty of the peristomes of Mosses becomes very impressive. If the sole end be the protection and extrusion of the spores in the proper weather respectively, why is there this infinite wealth and variety of form and of colour? The question can be asked, but hardly can be answered: and the mind of the beholder is left, as it so often is when contemplating the richness of Nature, in a state of admiration and wonder and ignorance. "Rerum natura tota est nusquam magis quam in minimis."

(*To be continued.*)

THE CHEMICAL ELEMENT CARBON.

By VAUGHAN CORNISH, B.Sc., F.C.S.

THE older conception of a chemical element was that of a property, or plexus of properties, which being common to several substances was regarded as a *principle* existing in all those substances. In some of these old *principles* we can recognize the present chemical elements. But whereas we now think of the elements as the undecomposed residues of natural substances and as being forms of ponderable matter essentially uncreatable and indestructible, the earlier conception of a chemical principle was that of a constant *property* possessed by certain forms of matter, *e.g.*, the combustibility which is a property common to all substances of animal and vegetable growth. It is the dependence of certain properties on the presence of certain kinds of material substance which aids us in many cases to connect the older chemistry with that of the present time, by identifying some of the so-called *principles* of the seventeenth century with the corresponding *elements* of the nineteenth century chemists.

A short history of the study of Carbon will serve to show the gradual evolution of certain important modern ideas with regard to chemical elements.

When vegetable or animal materials are heated with a limited access of air the result is that they are charred. Materials differing in almost every respect, except in their being formed in the processes of animal or plant life, agree in this—that when treated as above described they yield a *char*. The word *carbo*, formed from the same root, appears to have been used in the Augustan age in the same sense, namely, to designate the char left by the partial burning of animal and vegetable bodies. It was usually applied to the char obtained from wood or wood charcoal. In the more modern use of the Latin tongue *carbo* generally means coal, the vegetable origin of which is readily recognized.

The first important generalization in the history of chemistry is that contained in the theory of Phlogiston, which retained its hold upon the minds of chemists till the later years of the eighteenth century. The merit of this system lay in the fact that the various phenomena of oxidation were for the first time grouped together and referred to the same agency.

The change of properties undergone by metals on calcination was a favourite subject of study with the phlogistic chemists. An equal share of their attention was devoted to the means of restoring to calxes (or oxides) the metallic properties. The phlogistic chemists recognized that the process of calcination of metals was essentially a process of burning. Now the char or substance left by the partial burning of animal and vegetable bodies constitutes the most generally applicable material for the preparation of the metals from their ores, or from the calxes. The calx or ore is mixed with charcoal, or with the analogous material coal, and these being heated together, the charcoal or coal is consumed, to all appearance ceases to exist, and a metal is produced from the calx.

It is to be observed that these changes only take place at a high temperature, and that the materials must therefore be heated. Now the primitive method of heating is to kindle a mass of vegetable matter such as wood, burning the material with a free supply of air. It was, however, found that for metallurgical operations it was more advantageous to employ the prepared material charcoal, instead of the crude material wood. In metallurgical operations the charcoal (or the analogous material coal) plays two parts, each of which is essential to the reduction of the ore or calx. The first of these functions is the production of a high temperature, the second that of acting on the heated ore so as to form a metallic substance. The charcoal employed as fuel has undergone combustion, and the charcoal mixed with the ore has apparently imparted to the ore the power of burning, that is to say, has formed from it a metal which is a material capable of burning. Coal, coke, wood charcoal, animal charcoal,

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lampblack — in short, all the various forms of char — appeared to possess in an eminent degree what was termed the "principle of combustibility." The word *Carbone* was employed to denote the combustible *principle* contained in the various forms of char. The principle thus named is represented in terms of our modern views by what is known as a chemical element, and to this element the name *Carbon* is given.

The phlogistic chemists made no real advance towards explaining the reducing action of the various bodies containing the principle or substance Carbon. This explanation was reserved for Lavoisier, the great opponent of the phlogistic system. Previous to the work of Lavoisier, Black had prepared and studied the gas called "fixed air" (carbonic acid), which is obtained by the action of an acid on limestone. Lavoisier found that the same gas is formed when charcoal is burnt, and he further proved that in this process a constituent of the air (oxygen) unites with the charcoal, the fixed air or carbonic acid formed being composed of *Carbon* and oxygen. Lavoisier likewise proved that in calcination the substance he termed oxygen is abstracted from the air and combines with the metal, forming a calx or oxide. Finally, he showed that if a calx be heated with charcoal in the absence of air carbonic acid is formed, showing that the change which has taken place consists in removing the substance oxygen from its combination with the metal.

From this series of experiments it was evident that the *reducing* function of any form of char is better expressed by saying that the substance Carbon removes the substance oxygen than by saying that the combustible principle Carbon imparts to a calx the principle of combustibility. These two modes of expressing the same facts may be represented thus—

Calx *minus* the substance Oxygen = Metal, and
Calx *plus* the principle of combustibility = Metal.

From the date of Lavoisier's discovery the formation of carbonic acid has been invariably employed for detecting the presence of the element Carbon. Carbonic acid gas is readily recognized even in the smallest traces by the well-known property of throwing down a sediment of chalk when passed into lime water. Any substance which on burning forms carbonic acid contains, we say, the element Carbon. The amount of carbonic acid formed, which can be accurately determined, supplies the best means of estimating the weight of Carbon contained in any material. If a material when completely burnt yields carbonic acid *and nothing else* then we say that the material is composed wholly of the element Carbon. The purest wood charcoal when completely burnt yields no gas except carbonic acid, and no residue except a very small quantity of ash in which can be recognized those mineral ingredients which the plant from which the charcoal was prepared originally derived from the soil. Wood charcoal consists therefore almost wholly of Carbon. Any other form of char when freed as far as possible from foreign matter (*e.g.*, by drying to remove moisture) possesses the same, or nearly the same, physical constants as wood charcoal. The specific gravity is about 1·9. The materials are amorphous, or devoid of crystalline form.

It was not long after his discovery of the composition of carbonic acid that Lavoisier, in conjunction with certain other chemists, experimented on the combustion of the diamond. The diamond was placed in a glass vessel containing air, and the rays of the sun were concentrated to a focus on the diamond by means of a powerful lens. The diamond was by this means heated sufficiently to burn in the air of the vessel, and the gas evolved was collected over mercury, and tested. It was found to be carbonic acid. Soon afterwards (1796) an English chemist showed that the amount of carbonic acid formed by burning equal weights of diamond and of charcoal is the same, a conclusion which has since been repeatedly verified with the superior accuracy of modern methods.

Both diamond and charcoal are therefore said to consist wholly of the element Carbon.

In the year 1800, a third substance was added to the list of substances known as forms of Carbon. The mineral plumbago, or graphite, was formerly regarded as identical with molybdenum (a metallic sulphide), the appearance of each being similar, and both possessing the property of marking paper with a black streak, whence the name graphite (γράφω to write). It was shown, however, by Mackenzie, that graphite burns with formation of carbonic acid, the amount formed from a given weight of the material being the same in the case of charcoal and of diamond. Here, then, we have a third *form of Carbon.*

These three substances differ, in the first place, in certain important physical characters. The specific gravities are different, diamond standing highest in the list, and charcoal lowest. Diamond crystallizes in the regular or cubic system, graphite in the hexagonal system, whilst charcoal has no crystalline form or structure.

But it is not only in physical properties that the three substances differ; they differ to a certain extent also in chemical character. The temperature at which diamond burns is much higher than that at which the combustion of charcoal takes place. Thus, although oxygen unites with either substance, and in each case forms the same product, yet the readiness with which this combination takes place is very different in the two cases. In other words, the chemical relations of diamond and of charcoal, with respect to oxygen, are by no means identical. Again, graphite differs from either of the foregoing, in that the combined action of nitric acid and potassium chlorate convert it into a peculiar acid, a solid substance, known as graphitic acid.

In spite of these marked differences, chemical as well as physical, it is the universal practice to denominate all three substances, diamond, graphite, and charcoal, as "forms of the element Carbon," or *allotropic* modifications of Carbon. It must be confessed that the present phraseology is not as clear as might be desired, and is constantly a stumbling-block in the way of the tyro in chemistry. Set a schoolboy to write an essay on "allotropy" (or the existence of elementary substances in different "forms"), and he will choose as his example the element Carbon. He will begin by pointing out how widely different are the substances diamond, graphite, and charcoal, and will wind up his essay by saying that, notwithstanding these differences, "they are really the same thing—Carbon."

The source of confusion must be explained by reference to the atomic theory. We possess a great mass of evidence to show that what we observe in any chemical process is in fact the sum total as observed on the large scale of a number of phenomena, all precisely alike, occurring between the ultimate particles or chemical atoms of the substances. All the thousands of known substances are formed by various combinations of atoms of a comparatively small number of chemical elements. All the atoms of any one chemical element are exactly alike, but are different (*e.g.* in their mass) from the atom of any other element. A substance containing more than one kind of chemical atom is termed a compound substance or chemical compound. A substance containing only one kind of chemical atom is termed an elementary substance. Such a substance is diamond; it is formed wholly from atoms of one kind, from Carbon atoms. Charcoal likewise is

formed wholly of Carbon atoms. All Carbon atoms are, we believe, alike; but this by no means necessitates the identity of substances composed wholly of those atoms. We must look upon diamond and charcoal as *structures* both formed of the same material (the Carbon atom), but built up in different ways.

Regarded in this way the subject of *allotropy* is easy to understand, as easy as it is to understand, for instance, that a certain Jacobean house in Staffordshire is not a Norman castle though built of the sandstone blocks which once formed the feudal fortress originally standing on the same site. Sandstone is the matter of which both the house and the castle have been built, and in the same way diamond, charcoal, and graphite are constructed wholly of Carbon atoms; but to say simply and without qualification that diamond is Carbon, and that each of the other two substances is Carbon, is to employ language somewhat loosely and in a way which undoubtedly leads to confusion.

WHAT IS AN ANT?

By E. A. BUTLER.

OF late years the Ant, as everyone knows, has become the pet of the scientific world, and, to some extent, of the unscientific also. The fierce light of publicity has been brought to bear upon these little creatures, and upon their secret and subterranean doings. The laws which govern their communities, their common labours, their wars and foraging expeditions, their individual intelligence as manifested in their power of distinguishing their friends and detecting aliens, their vision and perception of colour, their senses of smell and hearing, their devotion to their young, their development and the duration of their life—these, and other such items, have been made the subject of observation and experiment, and the results have been eagerly read and discussed even in the daily press. But notwithstanding that Ants have become so famous, and their doings have been so minutely chronicled, there does not seem to be in the minds of the public generally a very distinct idea as to the identity of the creatures themselves, or, in other words, as to what insects are Ants and what are not. When an entomologist shows his collections to non-entomological friends, if he happens to have amongst his specimens any little dark-coloured, long-legged, wingless creatures, he is pretty sure to hear the suggestion hazarded in an inquiring tone that these must be Ants. Such a supposition will, it is likely, be quite as often wrong as right. The popular conception of an Ant is no doubt derived from the little black or dark-brown wingless individuals which one meets with everywhere, in our gardens and around our houses, quite as much as in the fields, lanes, or woods. But a conception which is formed merely by a random glance at such minute objects in rapid motion, and seen without the help of a magnifying glass, cannot but be vague in the extreme, and it is not surprising, therefore, that mistakes should frequently be made. The scientific idea of an Ant must be a good deal broader as well as a good deal more definite than this popular conception, and it is our purpose in this paper to show what are the distinctive characteristics of Ants, and how they can be distinguished from the numerous other insects to which they bear a superficial resemblance.

One cannot pronounce off-hand of any little dark-coloured, wingless, running insect, that it is an Ant, and, on the other hand, many true Ants would be neither dark-coloured nor wingless. Certain definite structural characteristics, which are accompanied with certain well-marked peculiarities of economy and habits, serve to distinguish Ants from other insects. Though what we have to say in this paper is intended only to apply to British Ants, it will be as well at the outset to correct a possible misapprehension, and to observe that there is a well-known tribe of insects which inhabit tropical regions, and no members of which are natives of Britain at all, that have unfortunately been called Ants though they are of an entirely different nature, whereby has resulted great confusion of popular zoological ideas. The insects in question are the so-called "White Ants," better named Termites, whose ravages are one of the greatest trials and annoyances of tropical countries. These destructive insects we have, quite apart from geographical limitations, nothing to do with here; zoologically they are not Ants at all: their structure is very different from that of the true Ants, and in many important respects their economy and habits are also strikingly dissimilar. The reader will therefore be good enough to exclude these creatures from his thoughts, and bear in mind that nothing that is said has any reference to them. The insects whose characteristics we have to consider are those which in this country are known as Ants or Emmets. We have between twenty and thirty kinds of them in this country, and these differ greatly in colour, ranging from the palest yellow, through various shades of red and brown, to deep jet black. Nevertheless there is a family likeness about them that renders them easily recognizable when once the distinctive points are known.

The first of these is to be found in the form of the insect. There is a large head (see Fig. 1), which is more or less abruptly cut off square behind, where it is often at its broadest. The head contains within it the brain and the muscles that move the jaws, in addition to the commencement of the digestive tract; and when we remember, in conjunction with this, the high degree of intelligence Ants manifest, and the muscular strength that is required for the hard work the jaws have to do, in fighting, in excavating, and in carrying heavy weights and unwieldy masses, often larger than the insect itself, we shall see very good reasons for the great size of the head, and shall naturally expect to find it, as is really the case, largest in those members of the community which have to do most of the work, whether mental or physical, viz. in the workers. The head is succeeded by a hump-backed thorax, often the narrowest part of the body, and showing very distinctly its composition out of three distinct segments. Then follows the abdomen, which is often extremely small in proportion to the other parts. Now it is in the construction of this part of the body that one of the most characteristic Ant-features is to be found. The front part of the abdomen is drawn out into a kind of thin stalk, which forms the connecting link between it and the thorax. But as this is the case with the majority of the Hymenoptera, to which order the Ant belongs, and gives them the narrow-waisted appearance which is familiar in wasps and ichneumon flies, it is not in the mere presence of this petiole, as it is called, that we find the distinguishing feature, but rather in its peculiar

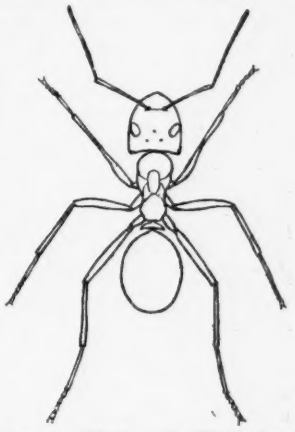

FIG. 1.—Worker of Wood Ant (*Formica rufa*).

form. The petiole is raised on its upper surface into one or two prominences, which have been called "nodes" or "knots"; the form of these, and the remarkable outline they give to this part of the insect, can best be seen by a side view (Fig. 2). The presence of these "knots" is one of the features by which an Ant may be recognized at once, and more than that, their number will determine to which of our two chief families any given specimen belongs. Omitting two rare insects which represent the family *Poneridæ*, but which are not likely to come in the way of the ordinary observer, and which, for practical purposes, may therefore be disregarded here, our British Ants are arranged in two families, the *Formicidæ* and the *Myrmicidæ*; the former, which contains the black and brown species we find in our gardens and streets, having only one "knot," and the latter, which contains the red ones, having two. The petiole is movably jointed to the hinder part of the thorax, and hence the abdomen as a whole can be bent about this joint as upon a hinge. The extra joint in the petiole itself, in the *Myrmicidæ*, gives still greater mobility to the tiny oval abdomen, at the end of which is situated the sting, and apparently gives these creatures greater freedom in the use of that weapon than if they had only one knot. The *Formicidæ* do not sting, and are satisfied with an abdomen which is not capable of quite such extended movement. Though they do not actually sting, however, they are provided with an abundant supply of poison (formic acid), which they can eject at pleasure, and which is thus instilled into wounds made by the mandibles. If, for example, a nest of the great "Wood Ant," whose huge piles of sticks and fragments are familiar objects in woods, be disturbed, a strong smell resembling that of vinegar is perceived, and if the hand be brought near the opening, the insects rear up on their hind pairs of legs, open wide their jaws, tuck their abdomen between the hind legs so that its extremity points forward, and from this eject the poison with great force. The operator's hand soon experiences a smarting sensation resulting from the battery of formic acid brought to bear upon it, and if it then be touched with the tip of the tongue, a sharp sour taste will be observed. Sir John Lubbock states that he has experienced the effect of the formic acid upon the hand when held at a distance of as much as eighteen inches. If the head be held over a disturbed nest, a little distance above it, the atmosphere is found to be so impregnated with the fumes of the acid as to be almost overpowering.

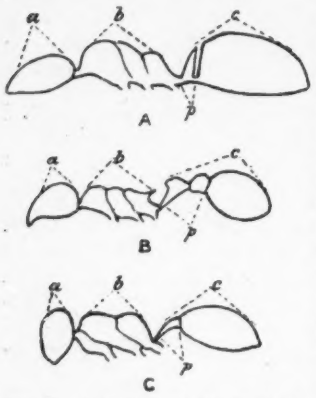

Fig. 2.—Side view of bodies of (A) *Formica rufa*, (B) *Myrmica ruginodis*, (C) *Pezomachus zonatus*. *a*, head, *b*, thorax, *c*, abdomen, *p*, petiole, with one "knot" in A, two in B, and none in C.

Amongst the ichneumon flies and allied insects, there are to be found many species which are wingless, and which, as they run about on the ground, amongst dead leaves or other rubbish, or over the foliage of living plants, look a good deal like Ants. They may, however, be distinguished by an appeal to the knotted petiole test. The petiole is, indeed to be found in such insects, as their narrow waist at once testifies, but it has not the characteristic knots, as a glance at the accompanying figure (Fig. 2) will show. The insect whose body is represented in profile, and a portrait of which is given in Fig. 3, is a parasite on spiders, and has no connection with Ants, leading as different a life as could possibly be imagined.

The second feature of importance in distinguishing Ants from other insects is to be found in the antennæ. These are always of the same type. There is a little roundish joint by which they are attached to the head in the front of the face (Fig. 1). This is succeeded by a long slightly-curved shaft, occupying about a third of the length of the whole antenna; this is called the "scape." This, again, is succeeded by a string of small joints, the number of which depends upon the species and the sex; sometimes these increase in width a little before the tip, and so give a club-shaped appearance to this flexible part, which, from its flexibility as contrasted with the stiffness and rigidity of the scape, has been called the "flagellum" (*Latin*—whip), the lash, of which the scape is the handle. Now, bees and wasps also have antennæ constructed like this, but as they are never wingless, they are not likely to be mistaken for Ants, notwithstanding their "elbowed" antennæ. A greater difficulty will be felt in distinguishing winged Ants from wild bees, but here the scale or knot on the petiole will come to the student's assistance and settle the matter. If we look now at those other wingless Hymenoptera which are not Ants, we see that this antennal feature may again be used as a means of discrimination. It is true that in such insects the second oint of the antennæ is a good deal longer than any of the rest, and sometimes (especially in the very small species) even as long as in the Ants. In such cases the knotted petiole test must be applied. But in many instances the second joint is not nearly so long as in the Ants, and then the many-jointed terminal part is not placed at an angle to the rest, so that the antennæ do not become "elbowed" (Fig. 3). This is more evident in the living insect than in the dead one. The Ant's antennæ are carried pointing forwards, but with the flagellum set at an angle to the scape, like a human arm bent at the elbow, and then the whole organ and its two chief parts can be placed in the same variety of positions as the arm which it imitates; the antennæ of the *Pezomachus* above-mentioned, and other parasites are not carried bent in this way, but straight forward, and their tips are maintained in an incessantly quivering or vibrating condition, as the insect goes on its way. It is astonishing what differences of expression can be imparted to the head by the varying positions of the antennæ. The importance of this is seen when we bear in mind that an Ant's head, like that of any other insect, is covered with a hard, unyielding skin, any movement in which is absolutely impossible: all expression of the emotions, therefore, must be restricted to the movement of external parts, like the jaws and antennæ, and to the varying positions of the head itself; in fact, nothing more devoid of expression can be imagined than an Ant's head, apart from the jaws and antennæ; the fixity of the eyes and the bloated appearance of the head itself make it look as unintellectual as the helmet of a diver. And yet this expressionless object can have a strong semblance of an air of war-like courage and bold defiance, of intelligent appreciation and affectionate sympathy, of industrious

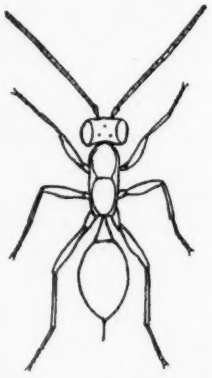

Fig. 3. — *Pezomachus zonatus*, a parasite on spiders; not an Ant.

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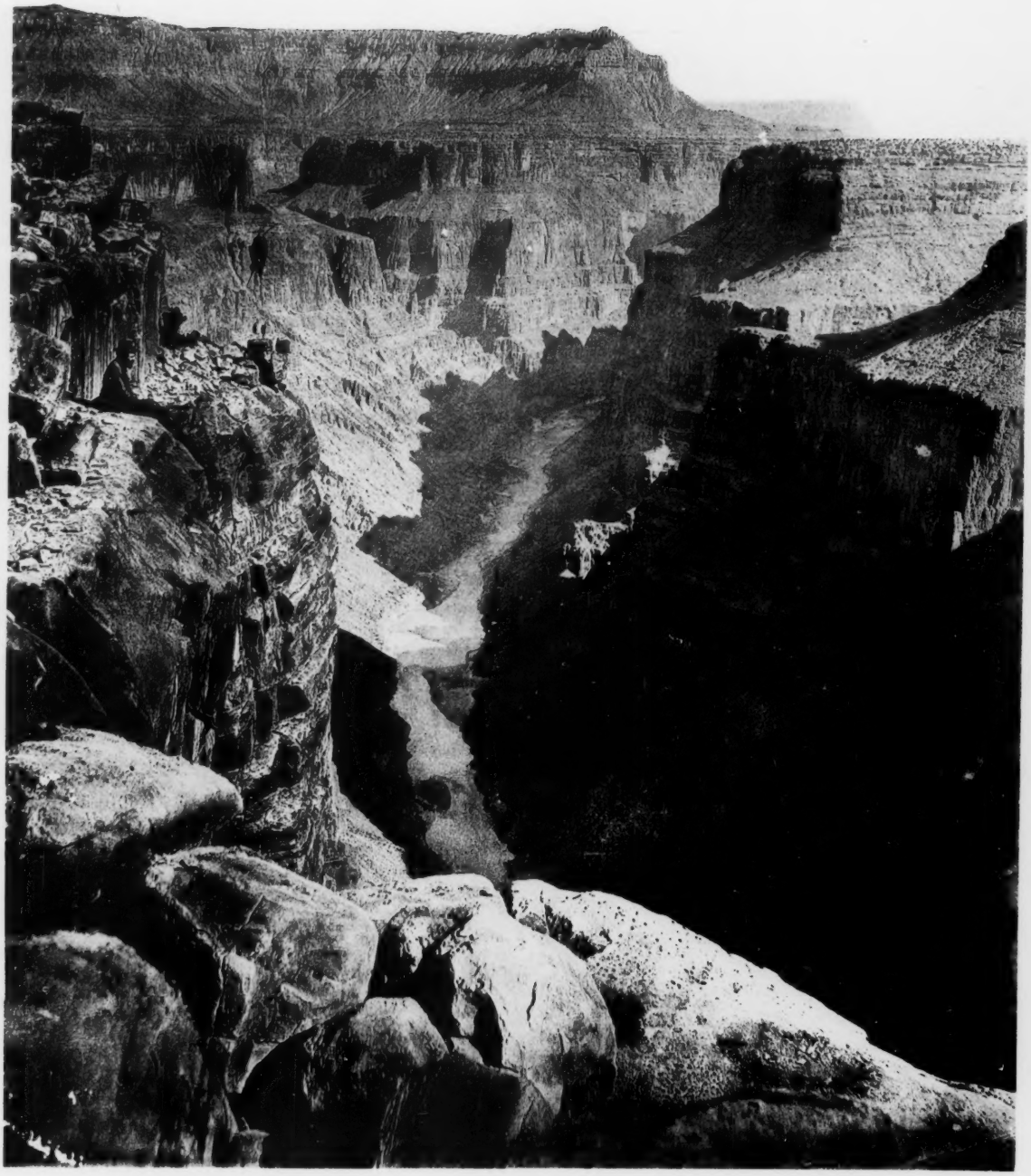

Inner Gorge of Toroweap, looking East from near the summit of the Cliffs.

The Automatic Engraving Company, Strode Road, Willesden Green.

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effort and fussy energy, imparted to it, simply by varying its position and by altering the attitude and motion of the jaws and antennæ.

Hitherto, we have been speaking only of the wingless forms of Ants, but these, though by far the most numerous, by no means constitute the whole of any given species. Every Ant exists in three forms, the male, the female, and the worker. Both the male and the female always have wings when first they assume the perfect form; the latter sex, however, retain them only till the marriage flight is over: they then voluntarily tear them off, so that in this sex the wings are only temporary appendages. These winged forms are seen for so short a time during any single season, that many people no doubt have never noticed them at all, and find it difficult to believe that such things exist. But even when one does see them, which will probably take place some fine day in August or September, it is often difficult to recognise them as having any connection with the wingless workers with which one is so familiar. It is clear that if two pairs of membranous wings, one large, the other small, be supposed to be added to a worker Ant, such an addition would of itself greatly alter the appearance of the insect. But this is by no means the only difference; there is also often a striking dissimilarity both with regard to size and colour, and the males, which are the smaller of the two, are frequently also much unlike their partners in colour and shape. For example, the little yellow Ant (*Lasius flavus*), which is abundant in many meadows and on heaths, making little hillocks, or taking advantage of the protection of a large stone or loose piece of rock, is yellow only in the worker; the male and female are both brownish black; or again, the little thin red Ant (*Myrmica rubra*), which is common everywhere, has a dark blackish brown male, which, in consequence of its wings and its deep colour, would be supposed to be a totally different insect from the worker, or the female. It is, then, only the workers that have no wings, or the females after they have mated, but then in this latter case the stumps of the wings may still be seen, whereas in the workers two little rounded points alone represent that portion of the larval structure which in the males and females developes into wings.

The development of wings in the male and female Ants has an important influence on the shape of the thorax, which adds another means of distinguishing the latter from workers when they have lost their wings. In the workers, the three segments of the thorax are not very unequal in size (Fig. 2), as each contains similar sets of muscles, viz., those for one pair of legs only; but when the wings are developed additional muscles are needed for these, and must be accommodated in the two hinder segments; hence the prothorax (first segment) becomes overlapped by the greatly enlarged mesothorax (second segment), which gives a still more humpbacked form to the insect, and the metathorax also (third segment) becomes enlarged. The enlargement is greatest in the mesothorax, as that segment has to carry the first pair of wings, which is by far the larger of the two, and is the chief instrument of flight. The wings are composed of transparent membrane, more or less clouded with a smoky tint, and strengthened by a few nervures, which enclose only a small number of cells.

Now the other wingless Hymenoptera with which Ants are often confounded do not exist in these three forms; all examples met with will be either males or females, no such things as workers being known, for a very good reason, viz.:—that there is no work for them to do. The insects do not form communities, but are, as already mentioned, piratical in habits, each managing its own affairs, which, in the case of the female, consist of little more than finding a suitable host in whose body she may deposit her eggs. These insects do not, as the Ants, couple in the air, and therefore never have any wings at all. It will be impossible, then, ever to find such insects in communities, or winged, in both of which respects they are to be distinguished from Ants. It should be observed, however, that some species, of which the females are always wingless, have winged males.

We are now in a position to answer our query "What is an Ant?" We have seen that Ants are Hymenopterous insects, which live in communities comprising three types of individuals, males, females, and workers; that the two former ones are winged and are to be met with only at a certain season in the year, but that the latter are never winged; that the antennæ are elbowed, and that the abdomen is attached to the thorax by a knotted petiole. And as they possess a poison gland and either a rudimentary or fully developed sting, they are referred to that section of the order called the "Aculeate" (sting bearing) Hymenoptera, which also contains bees, wasps, and some other insects. Lastly, they constitute a compact group of this section, to which the name Heterogyna has been given, in consequence of the great size and very different appearance of the females (*Greek:* dissimilar females).

THE CAÑONS OF COLORADO.

[*Second Paper.*]

By the Rev. H. N. Hutchinson, B.A., F.G.S.

(*Continued from page* 11).

CERTAIN important consequences follow from the rules about rivers, laid down at the end of our last paper. Thus, when rivers flow through or across mountain chains and plateaus, they must be older than these structural features. The elevation of a platform across the track of a river rarely diverts it from its course, because as fast as the ground under the river rises it cuts its way down through the obstruction. The region we are now considering offers a very complete illustration of the rules previously stated. We know that during the whole of Mesozoic time the watershed of the Colorado was submerged. In early Cainozoic (Lower Eocene) time it was a great freshwater lake. In due time this lake was drained, or emptied, presumably by the cutting down of its outlet as the country rose. By this process the present drainage system was begun, and every river must then have run in conformity with the features of the surface just exposed after rising above the waters of the lake, in which the Lower Eocene rocks were formed. But to-day we find that surface greatly deformed by displacements, and by erosion. The former made the "faults" and monoclinal flexures; the latter carved terraces out of the Eocene strata. The present courses of the rivers are not what they would have been if these features had then been in existence. They are entirely independent of them. They run in most cases against the slopes, and against the inclinations of the strata. They even cut through mountains and plateaus, enter cliffs and emerge from them; they flow over the monoclinal folds; they cross "faults" from the upthrow to the downthrow sides. As before stated, these facts can only be explained on the assumption that the rivers are older than the changes represented by all these structural features.

Many complex operations are involved in the evolution of the Grand Cañon, but the main factor is the erosion of its platform, and all the others are found to be bound up

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with it. Mr. Powell, in his popular narrative of "Explorations of the Colorado River," has employed the term "base-level of erosion" to express an idea which is of great importance in physical geography. He was the first to give it that definiteness which it formerly lacked. The idea is this: when a smooth country lies only a little above sea-level, erosion takes place at a merely nominal rate. The reason of this is obvious, for the slopes being very slight, the velocities, and therefore the transporting powers of the streams are so feeble that they can do no more than urge along the detritus brought down from highlands round the margin of the country. Soil formed on slopes or mounds of the expanse is slowly carried off. The erosion is then said to be at its "base-level," or nearly so. If any given region of the earth remained for a long time at the same height above the sea, it would at last come to this state, and erosion would practically cease. Many regions have done so in the past, but the greater portion of the existing land of the globe has been subjected to repeated up and down movements. Were it not for such movements, the balance between land and water could not be maintained. If elevation took place at a rate faster than erosion could keep pace with, the seas would become dry land, and if subsidence went on everywhere faster than deposition takes place in the seas, the continents would disappear, and the globe would be covered with water. Suppose any region to have reached a base-level of erosion, if it be depressed the sea spreads over it, and it becomes an area of deposition. If, on the other hand, it is elevated, new energy is imparted to the streams, and erosion takes place more rapidly, because their slopes are increased, and so their powers of corrosion and of transportation become much greater. In this way new topographical features are carved out and long rapid slopes or cliffs are generated, and we have seen that such features are vigorously attacked on account of their height. During the progress of the Grand Cañon, its anterior spaces have for a time been at a "base-level of erosion." Throughout the Quaternary, and most of the Tertiary period, it has been rising; and the elevation varied from 11,000 to 18,000 feet, but the movements have not been uniform. It appears to have alternated between periods of activity and repose. One period of repose probably occurred late in the Miocene, or early in the Pliocene period. While it lasted the great Carboniferous platform (in which the chasms occur) must have been planed down to a very flat expanse, bounded of course by the Terraces. But since then a general upheaval of several thousand feet has taken place, giving a fresh impetus to the river.

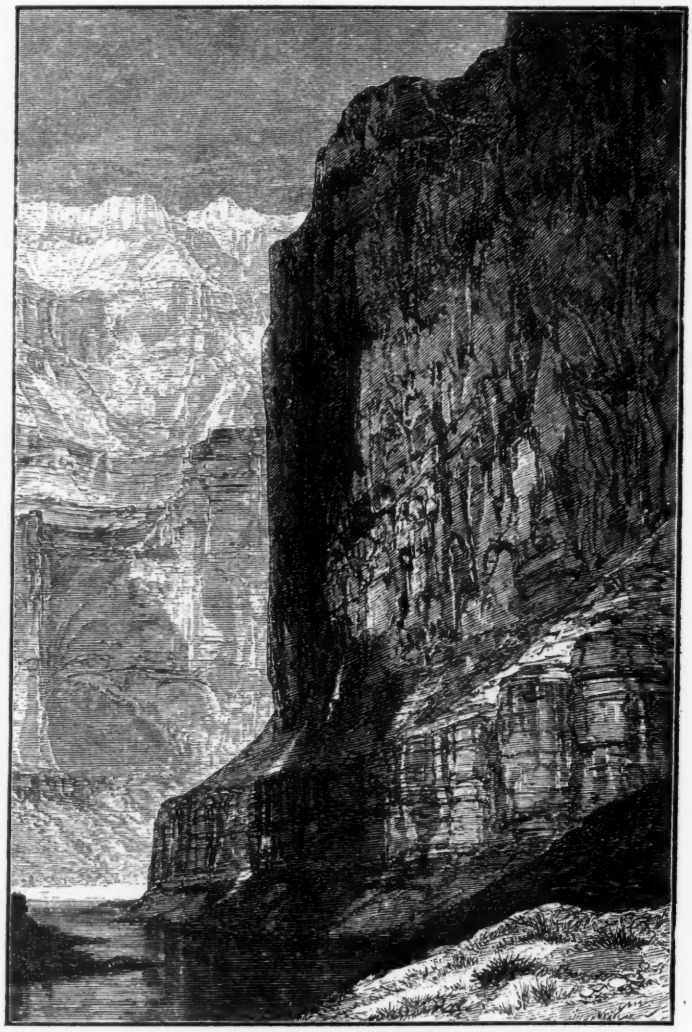


Fig. I.—The Marble Cañon.

Allusion may be made here to the volcanic phenomena of the Toroweap Valley, which runs along a "fault," whereby its western wall is made lower than the eastern one. Above and beyond is the Uinkarit Plateau, on the summit of which are a number of basaltic cones in perfect preservation. "Very many wide and deep floods of basaltic lava have poured over the edge of the plateau into the lower Toroweap Valley, and upon the great esplanade of the Cañon more than 1500 feet below, and spreading out into wide fields, have reached the brink of the inner gorge. Pouring over its brink, the fiery cascades have shot down into the abyss, and pursued their way many miles along the bed of the river. At one epoch they had built up the bed of the Colorado about 400 feet, but the river has scoured out its channel again and swept them all away, regaining its old levels; and is now cutting the sandstones below. The spectacle of the lava floods descending from the Uinkarit (plateau), as seen from 'Vulcan's Throne,' is most imposing."

There are about 120 distinct cinder-cones on the basaltic plateau of the Uinkarit, and probably many more volcanic vents. These often show a "linear arrangement" as if they were so many vents occurring along the course of a single fissure, but others, again, seem to be isolated in this respect. In Fig. II. volcanic dykes are seen (shaded dark). Then, the question arises whether they are connected with "faults," as might seem natural since "faults" are lines of weakness, up which steam and lava might find their way. Captain Dutton says, if there is any favoured direction taken by the volcanic forces, it would seem to be along the upthrow of the "fault," and a few miles from the line of dislocation. A truly wonderful displacement forms the western boundary of the Uinkarit

plateau. This is the great Hurricane "fault." Its full length is not known, but this enormous fracture extends at least 40 miles. In the southern wall of the Grand Cañon it appears with a displacement of about 1500 feet, throwing down the whole country to the west of it, and making a great cliff. The effect is as if a table were split across the middle and one side lifted up a foot or more. In one place the displacement is estimated at 12,000 feet; this is near the Valley of the Virgin, the Eocene strata on one side actually standing nearly 1000 feet lower than the much older (and therefore lower) Carboniferous rocks on the other side. Of the date at which this movement took place we shall speak later on.

The later geological history of the region of the Colorado plateau may now be briefly summed up. All the time when the great denudation was going on, by which the Mesozoic rocks overlying the platform of the Cañons were denuded and the Terraces formed, it may be concluded that the entire region occupied a level not much above the sea. It was at a "base-level of erosion." The rivers and streams no longer corroded their channels, but meandered slowly along the Carboniferous platform, dragging their burdens of soil along with them. This state of things was reached about the close of the Miocene period, and it lasted a considerable time. Then a new epoch of upheaval set in, and the work of cutting the Grand Cañon began. By this upheaval the country was hoisted up from 2000 to 3000 feet. Meanwhile a change of climate had taken place, the supplies of aqueous vapour failed, and the present dry time began, the rainfall being very slight. So most of the old streams dried up. Such streams as remained alive corroded their channels, but most of the platform suffered no more destruction than it does now by the slow waste that goes on. With this upheaval the Hurricane "fault" was developed, although it is possible that some slight displacement existed there already. If so, its magnitude was increased. At the same time the earliest volcanic outbursts took place. At length the uplifting paused for a time. Then the volcanoes ceased to work. The river for a time worked vigorously at the strata below it, the upward movement of the rocks having greatly increased both its corroding and transporting powers. But it quickly cut down until it found a new "base-level of erosion." Then corrosion ceased. During this second period of comparative repose the work of erosion was confined to the sapping of the newly-formed cliffs of the Cañon. The cliffs then began to recede away from the river; thus gradually was the broad avenue of the outer chasm made. When the cliffs of this outer chasm had receded two or three miles away from the river, another and more active period of upheaval set in. Once more the forces of elevation were brought into play, and this time the country was hoisted up more than before. Again the river began to corrode, or deepen, its channel. The displacements were increased in magnitude until the present "throw" of the "faults" was attained. These earth-movements affected the equilibrium between the internal and external forces at work on the so-called crust of the earth. The volcanoes became active again and poured out their lava streams. It was this second uplift that gave to the river the power to deepen its channel until it assumed its present condition. At present the elevating force, whatever it may be (and this is a question not yet settled), is inactive. Does it depend partly, as some think, on denudation? For it is conceivable that the removal of a considerable thickness of rocks from any given continental surface, may so diminish the downward pressure, due to their weight acting on the lower parts of the earth, as to disturb the equilibrium, and so force on an upward movement; but this is only a speculation. Anyhow, it seems that earth-movements and volcanic action are in some way connected together, for it is only during upheaval that volcanoes are active, and they are never associated with regions where depression is going on. The *active* volcanic regions of the globe at the present day are all regions of recent elevation, as may be seen by consulting a physical atlas.

This second upheaval was greater than the one which preceded it, amounting probably to 3000 or 4000 feet. The epoch at which it took place is no doubt a recent one, geologically. It probably began near the close of the Pliocene period.* At present

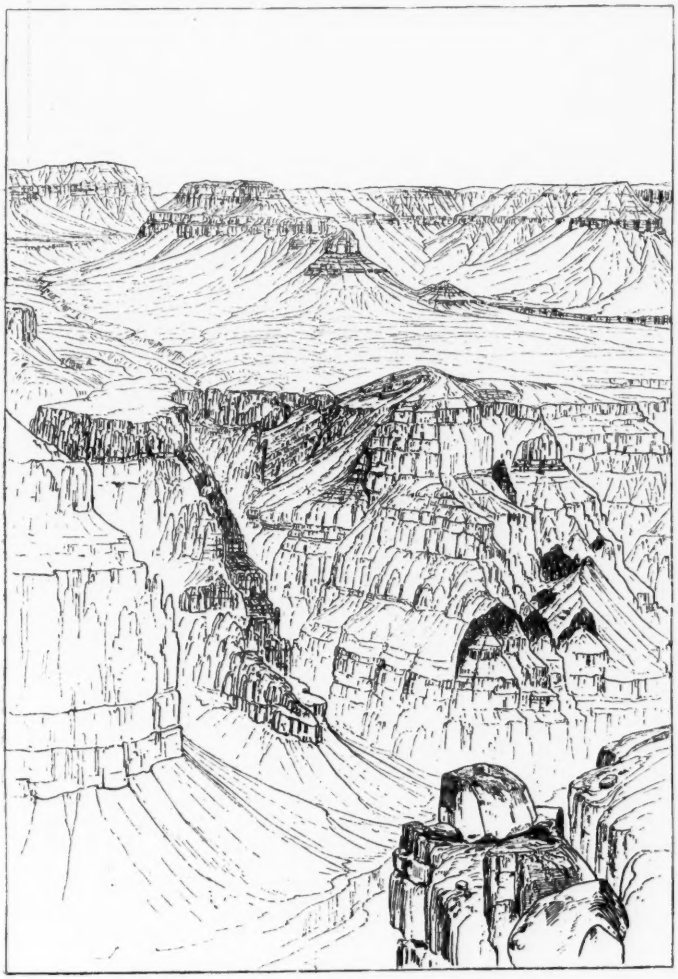


Fig. II.—Dykes in the Wall of the Grand Cañon.

* The Tertiary or Cainozoic era is divided by geologists into three periods. The oldest of these is the Eocene, then follow the Miocene and Pliocene.

no trace of movement can be detected, so it has probably ceased.

With regard to these displacements, there is some very interesting evidence which goes to show that these earth-movements took place slowly—in accordance with the "theory of uniformity" of the action of geological agents, which may be said to be the very basis of modern geology.

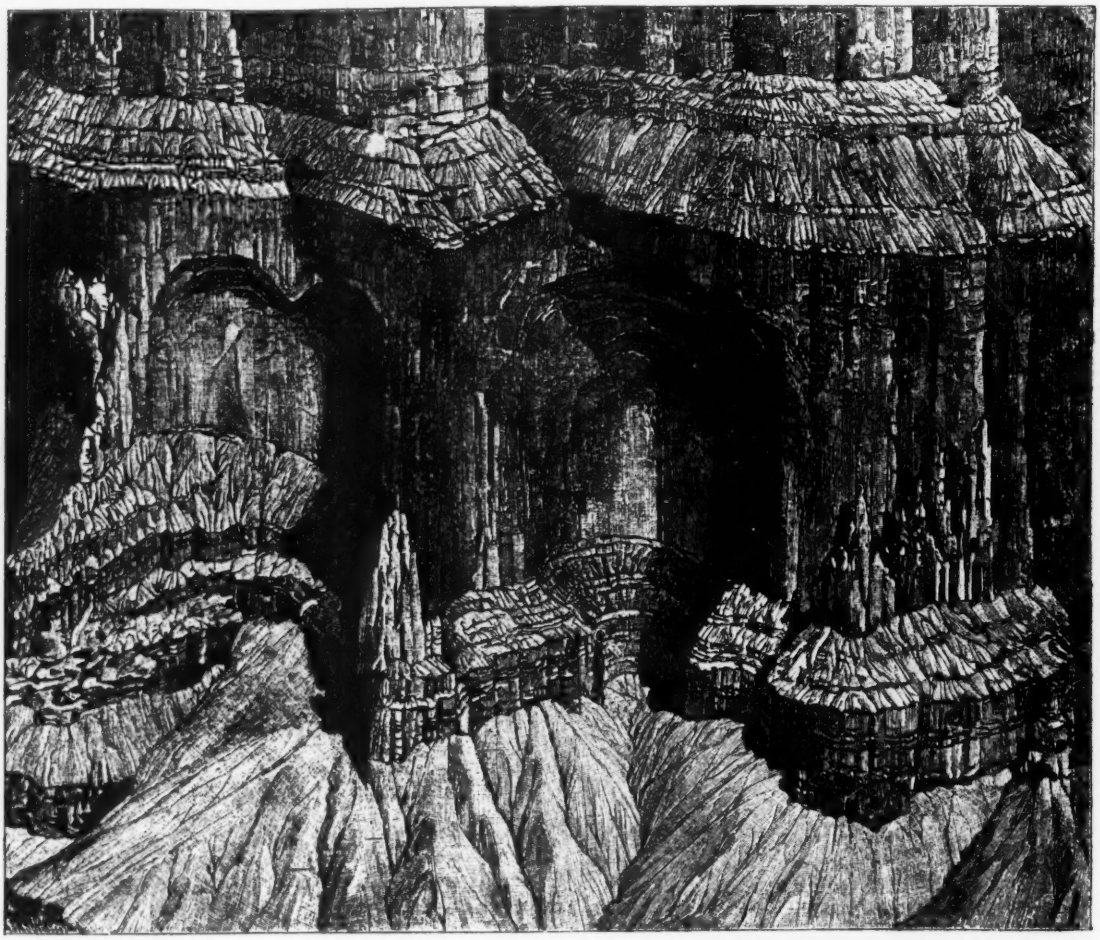


Fig. III.—Niches or Panels in the Red Wall Limestone, Grand Cañon District.

The old theories of sudden catastrophies and revolutions have been superseded by the teachings of Hutton and Lyell.

One of these displacements, known as the Toroweap "fault," exemplifies this point very clearly; for excepting the dislocation itself, the "faulting" does not appear to have been accompanied by an injury to the strata. There is no trace of any shattering, crumbling, or smashing of the strata. All looks as clean and sharp as if it had been cut with a thin saw, and the smooth faces pressed neatly together. The plane of the "fault" is nearly vertical. A careful study of its surroundings shows that it is of recent occurrence. None of the "faults" of this region seem to have been produced in a violent manner, but to have been gradually developed through long stretches of time, inch by inch, or foot by foot. Many facts go to prove the modern character of this "fault," as also of the other one already alluded to, viz., the Hurricane "fault." In each case they cut through lava flows, which are evidently pretty recent; therefore they must be subsequent to the time when the flows took place. Mr. Powell has shown a connection between monoclinal flexures and "faults." They often shade into each other, and it looks as if the bending of the strata went on until a point was reached when the tension could no longer be borne, and a split was the result. This might aptly be illustrated by the long, sharp, and clean fractures that take place in a sheet of ice after a number of people have been skating on it. At first the ice only bends, but after a time the tension becomes too great and results in fractures.

It is not necessary to describe in any detail the rocks of the Colorado plateau. Suffice it to say that the strata of each and every age are remarkably uniform over very large areas, especially in their lithological characters, and were deposited very nearly horizontally. In thickness and composition they are very persistent. The changes are always gradual. Local deposits, formed in small areas, are absent. They were all formed horizontally. The limestones are in great abundance in the Carboniferous rocks, but in the Mesozoic system limestones are rare. By far the greater part of the series of rocks exhibited in this

Map of the Cañon District. The shaded areas are covered with basalt. The residue of lava overflows from the N.E.

region is sandstone, in all its varieties. Sometimes it is quartzite, sometimes common sandstone in massive beds. Shales, however, abound in the Permian and Trias. These pass into the marly beds of the Cretaceous and Eocene. Thus we see that, while the strata are remarkably uniform in their horizontal range, they vary very much in vertical range, producing layers that differ very much in hardness, compactness, and solubility. Obviously the results of the attack of erosion on all these different rocks will be very different. These important facts have had a great influence in determining the architectural features of the cliffs and their profiles.

The upper tributaries of the Colorado River have their sources in lofty regions at a distance which are abundantly watered. But the main stream and its lower tributaries flow through regions which are exceedingly dry, so that nearly all the water flowing through the Grand Cañon comes from the highlands far away—the Uinta and the Rocky Mountains (*see* atlas). Though numbers of waterways open into the Cañons, very few of them carry living streams, the rest only convey spasmodic floods for a few days or hours when snows melt or showers and storms prevail. The rainfall is very slight. For the region draining laterally into the Glen and Marble Cañons it may be estimated at four inches per annum; but when rainstorms do come their consequences are very striking. The rills and washes are thick with mud and sand, and their waters loaded to their utmost capacity. There is nothing to hold the earthy matters together, so that the instant a rill is formed it is a rill of mud. Rills and streams gather together with marvellous rapidity, plunging furiously along. Although the rainfall on the middle and lower levels—about 5000 feet above sea-level—is on the whole quite small, the transporting power of such water as runs into the river is very great; a cubic yard of running water in the plateau country probably carries several times more sediment than the same quantity of water in rivers, such as the Mississippi, that run into the Atlantic. There are two causes for this remarkable difference: First, the comminuted *débris* of the plateaus due to "weathering" is not held together by vegetation, but lies loosely on the rocks and slopes of loose stones (called *taluses*); secondly, because the slopes are always very great, and we have already pointed out that the transporting power of streams is enormously increased by an increase in the declivity, as well as corroding power, it follows that much more solid matter is brought down. Soil and vegetation exist only in moist regions where a good deal of rain falls throughout the year, and they retard the work of erosion by forming a covering that protects the rocks below. They also tend to store up water and so equalize the flow of rivers and streams throughout the year, thus preventing the rapid floods we have alluded to above. The direct effect of increased rainfall is to increase erosion, but its indirect effect, through soil and vegetation, is to retard it. With regard to the slopes or gradients of the River Colorado, they vary considerably. The length of the part we are now considering is about 218 miles, and the average fall 7·56 feet per mile. But it varies from 3 or 4 to 21 feet per mile. The river is still sinking its chasm in the strata, though a great part of the river bed is over bare rock, and wherever this is so corrosion is proceeding rapidly. Its great cutting power is due to the large quantity of sand which it carries and the high velocity due to steep gradients. It only remains to notice a very curious phenomenon in the Red Wall Limestone, viz. the numerous niches or panels (see Fig. III.). Of these there are literally hundreds along the extent of the limestone front, and, as far as is known, they are seen in no other member of this series of strata. Captain Dutton says he is unable to explain the cause of this persistent phenomenon, and is very much perplexed by it. We can only suggest that they may have been dissolved out by the action of percolating water. It used to be thought that the Cañons might originally have been due to great cracks which the river has deepened and widened, but on reviewing carefully the mass of evidence brought forward in Captain Dutton's able monograph we find absolutely no trace of evidence for this idea. "Faults" and cracks could never have made such a great network of valleys.

Letters.

[The Editor does not hold himself responsible for the opinions or statements of correspondents.]

A LAW IN LOGARITHMS—AN ERROR OF VEGA'S.

To the Editor of KNOWLEDGE.

DEAR SIR,—It is of course widely known that the logarithms of numbers cannot be *always* calculated from tables by means of proportional parts, and that the degree of accuracy extends merely to a certain number of places. But it is *not*, I think, nearly so well known that the accuracy increases with the *squares* of the numbers. Thus suppose we are given ·0004340775 for the logarithm of 1001, and we calculate the logarithm of 10005 therefrom, by means of proportional parts, the result will be ·0002170387 . . . ; the arithmetical mean between the logarithm of 1000 and the logarithm of 1001. But the real logarithm of 10005 is ·0002170929 . . . ; and the logarithm calculated by proportional parts is too small by ·000000054 . . . If in the same way we calculate the logarithm of 100005 from that of 10001, we shall find the result is too small by ·00000000054; and so on, the error decreasing by two zeros for every increase of one figure in the number. The same law holds good with all intermediate numbers, and 0000000054 (8 zeros) is the error in the logarithm of 31625 if calculated by taking the arithmetical mean of the logarithms of 3162 and 3163. 3·162 . . . being the square-root of 10, 3162 is of course a geometrical mean between 1000 and 10,000; and therefore the error rightly has 8 zeros before the 54.

XUM

Curiously this law of error was overlooked by Vega in his splendid reprint of Vlacq's "Logarithms" to 10 places. Wishing to facilitate the correct calculation of intermediate logarithms to 10 places, he gave a table of what he called "second differences," which were to be added to the results taken by means of proportional parts. But omitting to take notice that these "errors" or "second differences" decrease inversely as the *squares* of the numbers, his table, throughout the greater part of it, makes the corrections greater than they should properly have been. T. S. BARRETT.

THE CAUSES OF THE ICE AGE.

To the Editor of KNOWLEDGE.

SIR,—I am sure that my friend Sir Robert Ball would not have made the charges which he has made against Sir John Herschel and Dr. Croll without good reason, though I am not aware of the passages on which it is based. But his article seems to imply that the general reasoning of both authors rests on this erroneous assumption, which is certainly not the case. Both assume (as indeed every writer on the subject must do) that each hemisphere receives more heat during its summer than during its winter. This is clearly implied, for instance, in 368*a* of Herschel's *Outlines*; but neither he nor Dr. Croll, I think, have anywhere attempted to compute the relative quantities of heat which we receive during our summer and our winter. This, however, was done in a very complete manner by Dr. Haughton in *The Transactions of the Royal Irish Academy* for 1881. His figures for the entire hemisphere agree pretty well with Sir Robert Ball's, but they vary with the latitude of the place.

Sir Robert Ball is probably correct in setting down the effects of the solar heat as equivalent to a mean elevation of temperature of more than 300° F. This elevation, if distributed in the ratio of 63 to 37, would give a mean elevation of 378° F. in summer and 222° F. in winter, with an annual range of 156° F. Of course, nothing like this annual range exists, the reason being that modifying causes (especially air-currents and ocean-currents) come into play. Now unless it can be shown that these modifying causes do not come into play in the case of the secular variation arising from changes in the eccentricity of the earth's orbit, the amount of the secular variation must be proportionally reduced. This, accordingly, is Dr. Haughton's conclusion. For lat. 50° he makes the annual variation of temperature 44·2° F., and the secular variation only 3·685° F., more than one-half of which will be represented by a summer elevation of temperature instead of a winter depression. Sir Robert Ball's reference to the large variation in the interior of continents is, I may remark, not much to the purpose, since it is not in the interior of continents but in islands and continental countries not far from the shore that the traces of glaciation are most evident; and in Siberia, for instance, where this great annual range of temperature exists, there is, generally speaking, no permanent snow-cap.

But behind all this lies the further question, Is such an unequal distribution of temperature favourable to the production of a *permanent* snow-cap? Dr. Croll's arguments on this point seem to me unsatisfactory. Sir Robert Ball (as far as I am aware) has not argued it at all. Suppose, as an extreme case, that the winter 37 per cent. of heat was distributed over 364 days in the year, and that we then received the remaining 63 per cent. on the 365th day. Would this 63 per cent. of the total heat be incapable of melting the snow-cap produced by the 364 cold days? I think not. In our latitudes 63 per cent. of the total annual heat would suffice to melt at least 50 feet of solid ice. It would no doubt have to raise the ice from a low temperature to freezing point before the melting commenced; but then, without a great increase in our rainfall or snowfall, it would not have 5 feet of ice to melt instead of 50. The snow might be more than 5 feet deep, but it would not be equivalent in amount to more than 5 feet of ice. In fact, a given quantity of heat would, it seems to me, produce its maximum melting effect when brought to bear in the shortest time. Suppose the heat received at a certain place is sufficient to maintain a constant temperature of 30° F. If the distribution is uniform, and the locality is sufficiently supplied with moisture, there will be a permanent snow-cap; but, unless the snowfall is very large, could I not melt the snow-cap once a year if allowed to distribute the heat as unequally as I chose?

This seems to me to be the most important question to be decided. As to Dr. Croll's theory of the diversion of the Gulf Stream, the fact that glaciation extends much farther to the south on the American than on the European side of the Atlantic affords pretty strong evidence that the Gulf Stream was not diverted during the Glacial Period.

Dr. Haughton takes occasion to point out that the secular range of temperature could not account for the miocene coal-beds of North Grinnell Land; but the possibility, or impossibility, of accounting for this and other evidences of a former mild climate in the Arctic regions is regarded by Dr. Croll as a crucial test of his theory. I remain, yours faithfully,

Dublin, Jan. 13th, 1892. W. H. S. MONCK.

[One of the difficulties to my mind in accepting the explanation of the cause of the Glacial Epoch suggested by Dr. Croll, and adopted by Sir Robert Ball, is that, if we could rely on such calculations, Mars should be permanently covered with snow, whereas we see its white polar caps wax and wane with the Martian summer and winter, proving, it seems to me, that there must be very potent modifying influences (such as the existence of a dense atmosphere) which upset the assumption that the mean temperature of a planet's surface may be assumed to vary inversely as the square of the planet's distance from the sun. Our atmosphere is probably not constant in quantity through geologic periods. Its gases are, we know, continually being absorbed by one set of chemical actions, and set free by others. The one set of actions may exactly balance the other, but if not, the atmosphere must be growing or decreasing in amount, and this would profoundly modify the surface temperature. So also would the elevation of an ocean bed which turned aside a tropical current such as the Gulf Stream.—A. C. RANYARD.]

Dr. MAX WOLF is continuing his photographic explorations of the Milky Way. In a letter received three or four weeks ago he informs us that several of his negatives show the tracks of meteors which have been observed to pass across the region photographed during the exposure. This gives the hope that in the future rich meteor showers may be observed photographically and the area of the heavens from which they radiate more exactly mapped than hitherto.

Prof. W. W. PAYNE, of Northfield, Minnesota, and Prof. George E. Hale, of Chicago, have published the first number of a new astronomical journal, which gives promise of occupying a very high place in astronomical literature. Prof. Hale states that he has undertaken the work at the instigation of his friend, Prof. C. A. Young, who

suggested to him the importance of establishing a periodical devoted to spectroscopy and astro-physics. He was loth to multiply the number of astronomical journals, and it was ultimately decided that the new periodical should be published in conjunction with the "Sidereal Messenger," which now changes its name to "Astronomy and Astro-Physics," and will appear ten times a year under the joint editorship of Profs. Payne and Hale. The subscription will be four dollars per annum.

Notices of Books.

Memory: Its Logical Relations and Cultivation. By F. W. Edridge-Green, M.D., F.G.S.; 2nd Edition. (Baillière, Tindall and Cox.) According to the author, memory is of two classes. There is sensory memory, dealing with the impressions received by the mind either from the external world or from its own processes, and motor memory, dealing with the motor impulses resulting therefrom—a classification based on the same principle as that of nerves into sensory and motor. Memory as a whole he treats as a distinct and definite faculty of the mind, having for its seat a limited portion of brain tissue; the sensory division he localizes in the *optic thalami*, and the motor in the *corpora striata*, the physiological basis of the memory being in each case a modification of the protoplasm of the cells of the nervous centre. Using the term "memory" in the broadest and most general sense of retentiveness, the faculty which is the foundation of all stores of knowledge and experience, he very fully and ably illustrates its differences in kind by reference to many of the most ordinary details of everyday life, showing that he has been a close and thoughtful observer of human nature. The idea which runs through the book is, perhaps, best expressed in brief by the following sentence: "The centre for sensory memory is so arranged that every impression received through a lifetime is registered in a definite position and order of sequence, from the first moment of a child's life to the day of his death, and all sensations, perceptions, and ideas received at the same time either form component parts of one impression or closely associated impressions." The memory, of course, consists in the revival of these impressions. The theoretical part of the subject is fully elaborated in a series of clearly written and closely reasoned chapters, which are extremely interesting reading whether one accepts the conclusions or not. But the book has more than a merely academic interest, great though that may be; Dr. Edridge-Green is intensely practical as well, realising as he does the extreme importance to its possessor, in the complex life of to-day, of a memory which can always be relied upon; and he applies his principles to the construction of a series of rules by the observation of which the memory may be cultivated and improved. These show a clear appreciation of the causes of the defects which are most commonly experienced, and though couched in somewhat technical language, they are sufficiently explained and illustrated by actual examples which are of familiar occurrence, and there will therefore be little difficulty in understanding their drift, and in putting them into practice. Like most other writers on memory, the author has a mnemonic system of his own, of special use as an aid in the memory of numbers; this, if somewhat cumbrous and troublesome, will no doubt yield good results to those who try it with enthusiasm, though we are of opinion that each person will generally profit most by a system either of his own devising, or at least of his own adaptation.

Handbook of the London Geological Field Class. (George Philip & Son.) This is a record of five years' work by the above class, which, under the presidency and personal conduct of Professor H. G. Seeley, has utilized Saturday afternoons during the summer months in studying geology at first hand. Excursions are made to localities which can be reached by a railway ride of not more than an hour from London; the professor explains on the spot the geographical and geological features of the locality visited, and the students, in their turn, under the professor's guidance, observe and chronicle matters of geological interest, collect fossils, &c., and are thus put in training which may, if they choose to follow it up, eventuate in their becoming practical geologists. By this excellent plan employment is found for the Saturday holiday, which is at once recreative, health-giving, and instructive; habits of scientific accuracy are encouraged by the constant appeal to Nature, which cannot but be of the highest educational value. The handbook is compiled by members of the class, under Professor Seeley's supervision, and in it we have abstracts of the professor's lectures and reports, between 80 and 90 in number, written by the students in description of the localities visited, and illustrated with sketches and sections from their own note-books. The whole forms a neat little pocket volume, which will be not merely of interest to the members of the class, but may be usefully employed as a guide-book by any amateur geologists who wish to study the cretaceous and tertiary deposits of the South East of England.

THE RELATIVE BRIGHTNESS OF THE PLANETS.

By J. E. Gore, F.R.A.S.

THAT the planets shine with very different degrees of brightness is a fact familiar, perhaps, to most people. The great brilliancy of Venus, when favourably situated as a morning or evening star, is well known, and has frequently given rise to the erroneous idea that a new celestial visitor had appeared in the sky. Jupiter, when in opposition to the Sun and high in the heavens, as it is in some years, also forms a brilliant object in our midnight sky, and it is closely rivalled in lustre by the "red planet" Mars, when nearest to the Earth, as it will be in the autumn of the present year. The difficulty of detecting the planet Mercury with the naked eye, owing to its proximity to the Sun, is well known. When seen, however, under favourable conditions, this planet shines with considerable brilliancy, but, as it can only be seen at its brightest for a few days in the morning or evening sky a little before sunrise or a little after sunset, and then only for a comparatively few minutes in the twilight, it generally escapes the observation of the casual observer. The "ringed planet" Saturn usually appears brighter than an average star of the first magnitude, and may be easily distinguished by its dull yellow colour. The light of this planet is of course considerably increased when the ring system is widely open, the bright rings being very luminous; but, when the rings are nearly invisible, as they are at present, the brightness of Saturn is much reduced. Uranus is just visible to the naked eye on a clear night when its exact position with reference to neighbouring stars is known, but Neptune is quite beyond the range of unaided vision.

These differences in the relative brightness of the planets are due to four causes: (1) The distance of a planet from the Sun; (2) the distance of the planet from the Earth; (3) the size of the planet; and (4) the reflecting power

of its surface, or the "albedo." Of these the first three are easily determined by observation, and a simple method of computing the relative albedos of the different planets forms the subject of the present paper.

The method of computation is as follows. The brightness of two planets will vary inversely as the square of their distance from the Sun, and *directly* as the size of the planets' discs as seen from the Earth, or, making due correction for their crescent and gibbous forms, as the square of their apparent diameters measured in seconds of arc. The results of this calculation will represent the relative brightness the two planets should have if both had the same albedo. If, however, one of them appears brighter than calculation indicates it implies that its reflecting power or albedo is greater than the albedo of the other. As the relative apparent brightness can be measured with a photometer, we have all the necessary data for calculation of the relative albedos.

The albedo is generally represented as a decimal fraction. This fraction denotes the proportion of light reflected compared with the amount received; the albedo of a surface reflecting *all* the light which falls upon it would be represented by unity. Probably, however, no such surface exists, the "albedo" of even freshly fallen snow being less than unity.

The difference of albedo in the planets is in some cases very striking. In 1878, when Mercury and Venus were in the same field of view of the telescope, Nasmyth found that Venus was at least twice as bright as Mercury. He compared Venus to clear silver and Mercury to lead or zinc. From photometric observations by Pickering and Zöllner, the brightness of Venus is nearly as great as if its surface was covered with snow, and Zöllner found that the surface of Mercury is comparable with that of the Moon, which has a small albedo. This difference of surface brightness is very remarkable when we consider that Mercury is much nearer to the Sun than Venus. If we suppose that the surface of Venus is covered with a cloudy canopy, as has been suggested, this cloudy covering would perhaps account for the planet's great reflecting power.

Owing to the uncertainty which exists as to the relative apparent brightness of Venus and Mercury as viewed with the naked eye, it is not easy to compute correctly their relative albedos. Olbers found Venus at its greatest brilliancy 19 to 23 times as bright as Aldebaran, but Plummer estimated it as nine times brighter than Sirius, which would make it 56 times brighter than Aldebaran. Mercury is perhaps about equal to Aldebaran when at its greatest brilliancy. I compared the planet and the star in June, 1874, in India, and found them about equal.

Assuming that when Venus is at her greatest brightness she is distant from the Sun 66 millions of miles, and that in this position she subtends an angle of 40 seconds of arc, and taking the corresponding quantities for Mercury as 28 millions and $8\frac{1}{2}$ seconds respectively, I find that Venus should appear about four times brighter than Mercury. Taking Venus as 20 times brighter than Aldebaran we have the albedo of Venus equal to five times that of Mercury. Zöllner found for Mercury an albedo of 0·13. My calculation would, therefore, make the albedo of Venus equal to $0{\cdot}13 \times 5$ or 0·65. Zöllner found 0·50. The data used in the above computation are too uncertain to yield an accurate result.

For the planets outside the Earth's orbit, let us take Mars as our standard. For this planet Zöllner found an albedo of 0·2672, or about double the albedo of Mercury. For the minor planets we have hardly sufficient data to enable us to compute their albedos; these little planets being so small that the apparent diameters of their discs cannot be accurately measured.

Comparing Mars and Jupiter, we have the mean distances from the Sun represented by the numbers 1·523 and 5·20. Their surfaces are therefore illuminated by sunlight in the inverse ratio of the squares of these numbers. That is, the solar illumination on Mars is to the solar illumination on Jupiter as the square of 5·20 to the square of 1·523, or as 27·04 to 2·32; and the apparent diameter of Mars at mean opposition may be taken at 18 seconds of arc, while that of Jupiter is 46 seconds. Hence the illuminated surface of Jupiter is $(\frac{46}{18})^2$, or 6·53 times that of Mars. The relative brightness of the two planets should therefore be $\frac{27{\cdot}04}{2{\cdot}32 \times 6{\cdot}53}$, or 1·78; that is, Mars should be 1·78 times brighter than Jupiter. Now Pickering found the stellar magnitude of Jupiter, when in opposition, to be 2·52, or about $2\frac{1}{2}$ magnitudes brighter than the zero of the scale of magnitudes, and that of Mars 2·25. This makes Jupiter 1·2823 times brighter than Mars. But we have seen that Mars should be 1·78 times brighter than Jupiter. Hence Jupiter is $1{\cdot}78 \times 1{\cdot}2823 = 2{\cdot}2825$ times brighter than it should be had it the same albedo as Mars. The albedo must therefore be $0{\cdot}2672 \times 2{\cdot}2825 = 0{\cdot}609$. Zöllner found an albedo of 0·72, but Bond computed that Jupiter emits more light than it receives from the Sun (Chambers' "Descriptive Astronomy," 3rd edition, p. 117). This would suggest that the planet shines with some inherent light of its own, a conclusion which has been also arrived at from other considerations.

In the case of Saturn the existence of the bright rings complicates the observations of the planet's brightness. Pickering's photometric measures make it about equal to a star of the first magnitude when in opposition and the rings invisible. Mars is therefore 3·25 magnitudes, or about 20 times brighter than Saturn. Now the relative distances of Mars and Saturn from the Sun are represented by the numbers 1·523 and 9·539. The squares of these are 2·32 and 90·99, which implies that the intensity of the solar light on Mars is 39·2 times that on Saturn. Taking the apparent diameter of Mars at 18 seconds and that of Saturn at 19 seconds, we have the apparent surface of Mars $(\frac{18}{19})^2$ or $\frac{324}{361}$ that of Saturn. Mars should therefore be $39{\cdot}2 \times \frac{324}{361}$, or 35·17 times brighter than Saturn. But it is only 27 times brighter. Hence the albedo of Saturn must be greater than that of Mars in the ratio of 35·17 to 27, or the albedo of Saturn $= \frac{35{\cdot}17}{20} \times 0{\cdot}2672 = (0{\cdot}47)$. Zöllner found 0·4981. I am inclined, however, to think, from my own observations, that Saturn, when in opposition and shorn of his rings, is slightly brighter than a star of the first magnitude. If this be so the albedo would have a somewhat higher value than that just computed.

Coming now to the planet Uranus we find the highest albedo of all the planets. Zöllner found 0·64, or slightly greater than that of Jupiter, but I find a still higher value. The relative distances of Mars and Uranus from the Sun are 1·523 and 19·183. The squares of these numbers are 2·32 and 367·99. Hence the intensity of the solar illumination on Mars is $\frac{367{\cdot}99}{2{\cdot}32}$, or 158·6 times that on Uranus. Taking the apparent diameter of Uranus at 4 seconds, and that of Mars at 18 seconds, as before, we have the area of the disc of Mars $(\frac{18}{4})^2$, or 20·25 times that of Uranus. Hence Mars should exceed Uranus in brightness $158{\cdot}6 \times 20{\cdot}25$ or 3211·65 times, if both planets had the same albedo. Now Zöllner found the stellar magnitude of Uranus to be 5·46; Pickering finds 5·56, and my own eye observations make it about 5·4. We may therefore safely assume its brightness at 5·5 magnitude. This gives a difference of 7·75 stellar magnitude between Mars and

Uranus, and implies that Mars is 1259 times brighter than Uranus. But we have seen that Mars should be 3211·65 times brighter if the surfaces of the two planets had the same reflecting power; hence it follows that the albedo of Uranus must be $\frac{3211\cdot65}{1259}$, or 2·55 times greater than that of Mars. We have, therefore, the albedo of Uranus $=0\cdot2672\times2\cdot55=0\cdot68$, or nearly equal to that of white paper, which is 0·70.

Let us now consider the planet Neptune, for which Zöllner found an albedo of 0·46. The relative distances of Mars and Neptune are 1·523 and 30·054. This gives the solar illumination on Mars 389·32 times that on Neptune. Taking their apparent diameters at 18 seconds and 2·9 seconds respectively, we have the result that Mars should be 14,996·6 times brighter than Neptune. Now Pickering found the stellar magnitude of Neptune to be 7·96, which makes Mars 10·21 magnitudes, or 12,023 times brighter than Neptune. Hence we have the albedo of Neptune $=\frac{14996\cdot6}{12023}\times0\cdot2672=0\cdot333$, a result in striking contrast to the albedo found above for Uranus. I think there can be no doubt that Uranus has the highest albedo of all the planets of the solar system. Comparing it with Jupiter I find, by the same method of computation, that the albedo of Uranus = albedo of Jupiter × 1·213. Hence with Zöllner's value of Jupiter's albedo, 0·62, we have the albedo of Uranus 0·75, a very high value indeed, exceeding that of white paper, which is 0·70, and pointing strongly to the conclusion that Uranus is in a highly heated condition, a conclusion which seems to be partly supported by the evidence of the spectroscope.

To further test the high albedo of Uranus, let us compare the relative brightness of Uranus and Neptune. According to Pro. Pickering's photometric measures, Uranus is 5·56 magnitude and Neptune 7·96. Uranus is therefore 2·4 magnitudes, or 9·12 times brighter than Neptune. The relative distances of the two planets from the Sun being 19·183 and 30·054, we have the intensity of the solar light on Uranus 2·4545 times that on Neptune. But the areas of the discs are as 4^2 to $(2\cdot9)^2$, or as 16 to 8·41. Hence, the brightness of Uranus should be $\frac{16}{8\cdot41}\times2\cdot4545$, or 4·67 times that of Neptune. Hence it follows that the albedo of Uranus must be $\frac{9\cdot12}{4\cdot67}$, or 1·9528 that of Neptune. Assuming Zöllner's value of 0·46 for the albedo of Neptune, we have the albedo of Uranus $=0\cdot46\times1\cdot9528=0\cdot898$ (!) Even with the low value of Neptune's albedo, which I have found, viz., 0·333, the albedo of Uranus would be $0\cdot333\times1\cdot9528=0\cdot65$, a value which still makes its albedo the highest of all the planets.

It is difficult to say what the albedo of the Earth itself may be. Possibly it does not differ much from that of the planet Mars. The Moon's albedo is rather low, 0·1736, according to Zöllner. It is, however, greater than that of Mercury, which seems to have the smallest reflecting power of all the planets.

With reference to the satellites, those of Mars are so small that we have no data for computing their albedos. Prof. Pickering's estimates of their diameter were made on the assumption that their albedos do not differ much from that of Mars itself.

Assuming a diameter of 3400 miles for the third satellite of Jupiter, the largest and brightest of the system, and the mean diameter of Jupiter itself at 87,000 miles, we have the area of Jupiter's disc 655 times that of the satellite. If both have the same albedo, Jupiter should therefore be 655 times brighter than the satellite. Now Pickering finds the stellar magnitude of this satellite to be 5·24. This makes Jupiter 7·76 magnitudes or 1271 times brighter than the satellite. Hence the albedo of Jupiter must be nearly twice that of the third satellite.

The diameter of Saturn's largest satellite, Titan, is somewhat doubtful, but assuming it at 3000 miles, and its stellar magnitude to be 9·43, as measured by Pickering, the diameter of Saturn being 72,000 miles, I find that the albedo of Saturn would be 2·2 times of Titan. This would make the albedo of Titan about 0·21, but owing to the uncertainty which exists as to its diameter this result must be considered very doubtful.

The satellites of Uranus and Neptune are so faint that no satisfactory results could be computed. For the satellite of Neptune Pickering finds a stellar magnitude of 13·82, or 5·93 magnitudes fainter than its primary. If we take the diameter of Neptune at 36,000 miles, and assume that its albedo is twice that of its satellite, I find that the diameter of the satellite would be about 3300 miles. Assuming the same albedo, the diameter would be about 2340 miles.

PERIODICAL COMETS DUE IN 1892.

By W. T. LYNN, B.A., F.R.A.S.

DR. S. OPPENHEIM has recently published in the *Astronomische Nachrichten* (No. 3064) the result of an investigation of the orbit of the fourth Comet of 1886, which was discovered by Mr. W. R. Brooks at Phelps, N.Y., on the 22nd of May in that year. He finds that the most probable length of its period is 5·6 years, and as it passed its perihelion in 1886 on the 6th of June, another will be due in the present month of January. Dr. Oppenheim thinks, however, that the Comet will not become visible unless the perihelion passage occurs considerably later in the year than this.

Failing this, the only known Periodical Comet due to return in 1892 is that of Pons-Winnecke, which was also last seen in 1886, when it was detected by Mr. Finlay at the Cape of Good Hope on the 9th of August, and passed its perihelion on the 16th of September. The first certain discovery of this Comet was made by Pons at Marseilles on the 12th of June, 1819, but it appears probable that it was observed by Pons himself early in February, 1808, though the observations made on that occasion were too few and doubtful (partly on account of the close neighbourhood of several nebulæ) to furnish the means of determining its orbit with any accuracy. It was after the return of Pons's Comet of 1819 in 1858, when it was re-discovered by Prof. Winnecke on the 8th of March, and passed its perihelion on the 2nd of May, that it was recognized as taking its place amongst the Periodical Comets, with period of about 5·6 years. It was not, however, seen at the next return, which must have taken place about the end of 1863, but was observed at the returns of the summer of 1869 and the early spring of 1875. At the return in 1880 it was unfavourably placed and again escaped observation, but (as already mentioned) it was observed again in the latter part of the summer of 1886, and will be due once more early in the present year.

In the year 1880 the late Prof. Oppolzer, of Vienna, made some calculations which appeared to indicate that this Comet was undergoing an acceleration of its period, and he suggested that this might be due to the effect of a resisting medium in space acting upon its motion, as Encke had thought he had obtained decisive evidence in the case of the Comet now always called by his name. But since the time of Encke the diminution of the periodic time in the latter Comet has proved to be not constant, so that the probability of the resisting medium explanation no longer holds. And in the case of the Pons-Winnecke

XUM

Comet, since its return in 1886, Dr. von Haerdtl, of Vienna, has made a re-investigation of its motions and found no evidence of any such effect of a supposed resistance. The suggestion, however, will give some additional interest to observations of the Comet at its approaching return to perihelion.

THE FACE OF THE SKY FOR FEBRUARY.

By HERBERT SADLER, F.R.A.S.

THE Sun's disc, when visible, should be examined for spots and faculæ. The following are conveniently observable minima of some Algol-type variables (*cf.* "Face of the Sky," for January). U Cephei.—February 4th, 5h. 55m. P.M.; February 9th, 5h. 34m. P.M. Algol.—February 2nd, 5h. 41m. P.M.; February 19th, 10h. 36m. P.M.; February 22nd, 7h. 23m. P.M. λ Tauri.—February 1st, 10h. 11m. P.M.; February 5th, 9h. 3m. P.M.; February 9th, 7h. 55m. P.M.; February 13th, 6h. 48m. P.M.; February 17th, 5h. 40m. P.M.

Mercury is a morning star during the first portion of the month, but owing to his great southern declination and proximity to the Sun is very badly situated for observation. He rises on the 1st at 6h. 46m. A.M., or 56m. before the Sun, with a southern declination of 22° 26′, and an apparent diameter of $5\frac{1}{2}''$, $\frac{82}{100}$ths of the disc being illuminated. On the 6th he rises at 6h. 52m. A.M., or 42m. before the Sun, with a southern declination of 21° 44′, and an apparent diameter of 6″, $\frac{86}{100}$ths of the disc being illuminated. After this date he is too near the Sun to be visible. Venus is an evening star, and is now becoming a conspicuous object in the western sky. On the 1st she sets at 7h. 47m. P.M., 3h. 1m. after the Sun, with a southern declination of 7° 14′, and an apparent diameter of $12\frac{1}{2}''$, $\frac{84}{100}$ths of the disc being illuminated. On the 29th she sets at 9h. 15m. P.M., with a northern declination of 7° 18′, and an apparent diameter of $14\frac{1}{4}''$, $\frac{76}{100}$ths of the disc being illuminated, and the brightness of the planet being rather less than one-half of what it will be at its greatest at the beginning of June and middle of August next. Venus is in conjunction with Jupiter at 10h. 14m. A.M. on the 6th, the geocentric distance separating the limbs of the two planets being only $18\frac{1}{2}''$, but the phenomenon occurs after sunrise in Europe. The two planets, however, will present a most beautiful appearance in the western sky in England on the evenings of the 5th and 6th. At 7h. P.M. on the 5th Venus will be about 38′ *s.p.* Jupiter, and at the same hour on the next evening she will be about $21\frac{1}{2}'$ *n.f.* Jupiter, the field of view on both evenings, with a low power, being a singularly pretty one. During the month Venus passes from Aquarius into Cetus, but without approaching any conspicuous star. Mars is, for the purposes of the observer, invisible; and as Uranus does not rise till after midnight at the beginning of February, we defer an ephemeris of this planet until next month.

Jupiter is still visible, close to the S.W. horizon, but he is so rapidly approaching the Sun that our ephemeris of him only extends over the first third of the month. On the 1st he sets at 8h. 16m. P.M., $3\frac{1}{2}$h. after the Sun, with an apparent equatorial diameter of $34\frac{1}{3}''$, and a southern declination of 5° 8′. On the 10th he sets at 7h. 52m., or 2h. 49m. after sunset, with an apparent equatorial diameter of $33\frac{3}{4}''$, and a southern declination of 4° 20′. The following phenomena of the satellites occur while Jupiter is more than 8° above, and the Sun 8° below, the horizon. On the 1st a transit egress of the shadow of the fourth satellite at 6h. 7m. P.M. On the 2nd a transit egress of the shadow of the first satellite at 5h. 52m. P.M. On the 4th a reappearance from occultation of the third satellite at 6h. 51m. P.M., and its eclipse disappearance at the same instant. On the 8th a transit egress of the second satellite at 5h. 54m. P.M.; while visible he describes a short direct path in Aquarius.

Saturn is an evening star, rising on the 1st at 9h. P.M., with a northern declination of 2° 19′, and an apparent equatorial diameter of 18·6″ (the major axis of the ring system being 42·8″ in diameter, and the minor 2·5″). On the 29th he rises at 7h. P.M., with a northern declination of 3° 3′, and an apparent equatorial diameter of 19·1″ (the major axis of the ring system being 44″ in diameter, and the minor 1·9″). The following phenomena of the satellites may be observed (the times are given to the nearest quarter of an hour):—February 2nd, $4\frac{1}{2}$h. A.M., Tethys, eclipse disappearance. February 3rd, 10h. P.M., Dione, eclipse disappearance. February 4th, 2h. A.M., Tethys, eclipse disappearance. February 5th, $11\frac{1}{4}$h. P.M., Tethys, eclipse disappearance. February 8th, $1\frac{1}{2}$h. A.M., shadow of Rhea in central transit. February 9th, $0\frac{1}{2}$h. A.M., shadow of Titan in central transit; 4h. A.M., Titan in inferior conjunction with centre of Saturn; 9·9″ south. February 10th, $5\frac{3}{4}$h. A.M., Rhea, eclipse disappearance. February 12th, 3h. A.M., Dione, eclipse disappearance. February 17th, $2\frac{1}{2}$h. A.M., shadow of Rhea in central transit; $4\frac{1}{2}$h. A.M., Titan, eclipse disappearance. February 19th, $4\frac{1}{2}$h. A.M., Tethys, eclipse disappearance. February 20th, 2h. A.M., Iapetus in inferior conjunction with the centre of the planet; Tethys, eclipse disappearance. February 22nd, 11h. P.M., Tethys, eclipse disappearance. February 23rd, 2h. A.M., Dione, eclipse disappearance. February 24th, $8\frac{1}{2}$h. P.M., Tethys, eclipse disappearance; $11\frac{1}{2}$h. P.M., shadow of Titan in central transit on Saturn. February 25th, $1\frac{3}{4}$h. A.M., Titan skirts southern limb of planet. February 26th, $3\frac{1}{4}$h. A.M., shadow of Rhea in central transit. During the month Saturn describes a short retrograde path in Virgo, without approaching any naked-eye star.

Neptune is still visible during the working hours of the night, rising on the 1st at 11h. 40m. A.M., with an apparent diameter of 2·6″, and a northern declination of 19° 48′. On the 29th he rises at 9h. 39m. A.M., with a northern declination of 19° 49′. During February he is almost stationary just north-west of ε Tauri, and about the middle of the month he will be observed to be about 80″ *n.f.* a $9\frac{1}{4}$ magnitude star.

There are no well-marked showers of shooting stars in February.

The Moon enters her first quarter at 9h. 39m. A.M. on the 5th; is full at 7h. 38m. P.M. on the 12th; enters her last quarter at 0h. 15m. A.M. on the 21st; and is new at 3h. 47m. A.M. on the 28th. She is in perigee at 9·2h. A.M. on the 1st (distance from the earth 226,765 miles); in apogee at 9·8h. A.M. on the 17th (distance from the earth 251,845 miles); and in perigee at 11·8h. A.M. on the 28th (distance from the earth 223,660 miles). The greatest western libration is at 2h. 46m. A.M. on the 9th, and the greatest eastern at 9h. 48m. P.M. on the 23rd.

Chess Column.

By C. D. LOCOCK, B.A.Oxon.

ALL COMMUNICATIONS for this column should be addressed to the "CHESS EDITOR, *Knowledge Office*," and posted before the 10th of each month.

The solution of the Four-Move Problem in the January number is necessarily withheld till its publication in the *Chess-Monthly*.

CORRECT SOLUTIONS have been received from W. T. Hurley, Giu. Pianissimo, M. B. (Jesmond), and A. Rutherford, who are to be congratulated on their success in mastering the intricacies of this most difficult problem. Duals, which were most abundant, of course did not count; otherwise even Giu. Pianissimo, whose analysis was most exhaustive and logical, would have missed one or two points. None of the four survivors have suggested an immediate division. An attempt therefore will be made to combine their various suggestions as far as possible, on the chance of a separation. For this purpose the following difficult problem is given; and, as an additional test, another problem has been posted (January 24th) to each competitor. In this latter problem the number of moves is not given, and duals, etc., in leading variations will each score one point, the same being deducted for each incorrect claim. Solutions of both problems should be sent in by February 9th, and the analysis in each case should be continued up to White's third moves.

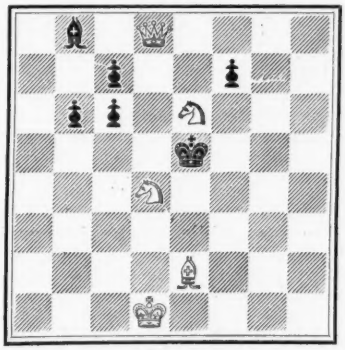
A. Rutherford.—Thanks for the correction. The mistake is explained below.

R. W. Houghton.—The problem was a very difficult one; to be beaten by it is no disgrace, though certainly unfortunate.

SECOND TIE PROBLEM.

[Also from the *Chess-Monthly* Tournament.]

BLACK.



WHITE.

White to play, and mate in four moves.

CHESS INTELLIGENCE.

The Steinitz-Tschigorin Chess Match in Havana began with the year. The match goes to the player who first wins ten games, draws not counting. Mr. Tschigorin won the first game; the second and third were drawn; Mr. Steinitz won the fourth, and, after another draw (56 moves), the sixth in 48 moves. The first four games averaged only 31 moves each. So far the Russian player has consistently adopted his favourite Evans Gambit, which Mr. Steinitz has defended by 7. . . . B to KKt5 and 8. . . . Kt to KB3, no longer apparently relying on his expensive defence Q to B3. In the other games he has played the common or German form of the Ruy Lopez, with the exception of the sixth game, which was a Two Knights' Defence.

The *Pittsburg Dispatch* announces a Three-Move Problem Tournament (direct mates only). Competing positions, with motto, solution, and sealed name and address, must be mailed (in Europe) not later than March 1st, and addressed to "*Chess Editor, Pittsburg Dispatch*, P.O. Box 463, Pittsburg, Pa.

The National Masters' Tournament of the British Chess Association will probably be held at the British Chess Club some time this month. The programme will be issued shortly.

The following is one of two Consultation Games, played on December 12th last by telephone, between the Liverpool Chess Club and the British Chess Club, London. The former were represented by Messrs. Burn, Dod, Rutherford, and Wellington; the London players being Messrs. Guest, Hirsch, Locock, and Mundell.

WHITE. (B.C.C.)	[Vienna Opening.]	BLACK. (Liverpool.)
1. P to K4		1. P to K4
2. Kt to QB3		2. Kt to KB3
3. P to B4		3. P to Q4
4. P × KP		4. Kt × P
5. Kt to B3 (*a*)		5. B to K2 (*b*)
6. P to Q4		6. P to QB4 (*c*)
7. B to Q3		7. Kt to QB3
8. Kt × Kt		8. P × Kt
9. B × P		9. Kt × QP
10. B to K3 (*d*)		10. B to B4 !
11. B × B (*e*)		11. Kt × B
12. Q to K2		12. Castles
13. Castles		13. Q to Kt3
14. P to B3		14. QR to Qsq (*f*)
15. B to B4		15. R to Q2
16. Q to K4		16. Q to K3 (*g*)
17. QR to Ksq		17. R to Ksq
18. Kt to Kt5 (*h*)		18. B × Kt (*i*)
19. B × B		19. Kt to K2
20. B × Kt		20. QR × B
21. Q to QR4 (*j*)		21. P to QKt3
22. R to K2		22. Q to Q4 (*k*)
23. R to Qsq		23. Q to Kt2
24. R to Q6		24. P to KR3
25. Q to B2 (*l*)		25. Q to Bsq
26. Q to Q3		26. Q to KKt5
27. R to Q8 (*m*)		27. Q to K3
28. R × R		28. R × R
29. Q to R6 (*n*)		29. R to K2
30. P to QKt3		30. R to Q2 (*o*)
31. P to KR3		31. P to KKt4
32. Q to R4		32. P to KB4
33. K to R2		33. K to B2 (*p*)
34. P to KKt4		34. P × P
35. P × P		35. K to Kt2 (*q*)
36. Q to K4		36. Q to Q4 (*r*)
37. Q × Q		37. R × Q
38. P to B4 !		38. R to Q2
39. K to Kt3		39. K to Bsq
40. K to B3		40. K to K2
41. R to R2		41. K to K3
42. R × P ch		42. K × P
43. R to Kt6 (*s*)		43. R to Q7
44. R × P ch		44. K to B3
45. R to B5ch		45. K to Kt3
46. K to K4		46. R × P
47. K to Q5		47. R to QKt7 (*t*)
48. R to B3		48. K to Kt4
49. K to B6		49. K × P
50. R to Q3 (*u*)		50. K to B5
51. K to Kt7		51. K to K5
52. R to R3		52. R to QR7
53. R to Kt3		53. K to Q5

And after a few more moves the game, which had lasted over seven hours, was given up as drawn.

Notes.

(*a*) 5Q to B3 is more usually played; Black has three good defences in QKt to B3, or Kt×Kt, or P to KB4. The London representatives selected the text move on the ground that it had not been much analyzed.

(*b*) If 5 . . . B to QKt5 White intended to reply 6. B to Q3!, or if 5 . . . B to KKt5, 6. Q to K2!, which is much better than 6. B to K2.

(*c*) An excellent reply, and we believe new at this stage. If instead 6 . . . Castles, White gets the superior game by 7. B to Q3.

(*d*) This move was the subject of much discussion, some of the players preferring 10. Castles. The simple move Kt×Kt would have given Black an isolated Pawn in return for their own, but was rejected as too unenterprising.

(*e*) If 11. Q to Q3, Kt×Ktch; 12. P×Kt, Q×Q; 13. B×Q, B×B; 14. P×B, Castles (QR); 15. K to K2, R to Q4; 16. P to KB4, KR to Qsq, with some advantage in position.

(*f*) After 14 . . . Kt×B, 15. Q×Kt, Q×P; White recovers the Pawn by 16. KR to QKtsq. Black prepares, instead, for doubling his Rooks on the King's file.

(*g*) The best way of defending the Knight, which now threatens to escape at Q3. Q to Kt3 was not so good; White might win a Pawn at once by Q to R4.

(*h*) With a view to simplicity, as there is not much chance of attack. B to Kt5 instead would be met by B to Bsq, threatening P to B3.

(*i*) Best. If 18 . . . Q to Kt3; 19. P to K6, B×Kt; 20. P×Pch, Q×P; 22. Q×Rch, Q×Q; 23. R×Qch, K to B2; 24. B×B and wins. Black's next move is also much better than Kt to Q3, which would ultimately leave the Knight out of play.

(*j*) The first of an interesting series of moves, by which White keep their King's Pawn indirectly guarded even against the constantly threatened . . . P to B3.

(*k*) If P to B3, 23. P×P. Black manœuvre to get their R at K2 and the Q behind it.

(*l*) Still preventing R×P. Q to KKt4 or Q to QB4 also have merits. Black's next move prevents Q to B5.

(*m*) A hasty move, made under pressure of the time limit. They should have played P to KR3 first, when the Queen would have nothing better than a return to QBsq.

(*n*) Black threatened P to B3, but perhaps K to B2 was a better defence.

(*o*) Preventing the escape of the Queen at Q3. If instead 30. . . . P to B3; 31. P×P, Q×R; 32. P×R, etc. After their next move White offered a draw, which was declined.

(*p*) K to Kt2 was probably better, *vide* their 35th move. On their next move 34. . . . P to B5 would of course be answered by 35. Q to K4.

(*q*) Before making this move Black in their turn proposed a draw, but withdrew the offer while White were consulting.

(*r*) If 36. . . . R to KB2; 37. R to Q2, R to B5; 38. Q to Kt7ch and draws; but the game should be drawn anyhow. After the exchange of Queens Black cannot attack the King's Pawn with both King and Rook.

(*s*) Playing now to win, but they should have been content to draw by K to K3. If then 43. . . . R to Q5; 44. R to R7.

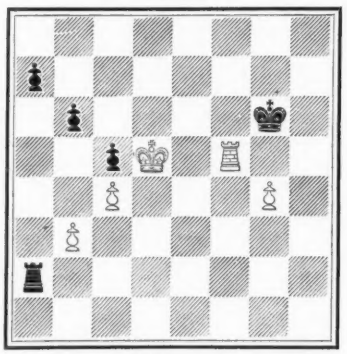
(*t*) They might play 47. . . . R to R6 (see diagram). If then 48. R to B3, P to R4; 49. K to B6? (not so good as K to K4 which seems to draw); 49. . . . P to R5; 50. P×P! (if 50. K×P, R×Pch wins); 50. . . . R×R; 51. K×P, R to B3ch?; 52. K to Kt5! (the only move. If 52. K to Kt7, R to B5 wins; or if 52. K×P, K to B2 wins). White now apparently draws at least by going straight on with the Rook's Pawn; but Black at move 51 should have played R to QR6! which apparently wins, as pointed out by Mr. Hoffer; but it is very difficult.

(*u*) R to QB3 would have saved a move, but the game is easily drawn.

Position after White's 47th move.

(LIVERPOOL.)

Black.



White.

(LONDON.)

In the other game, which will be given next month, the Liverpool representatives were Rev. J. Owen, and Messrs. Howard, Kaizer, and Cairns. The name of the latter gentleman bears a certain telephonic resemblance to that of Mr. Burn, hence our mistake last month in stating that Mr. Burn took part in both games. We are indebted to Mr. A. Rutherford, of Liverpool, for this information.

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